

# applied science and technological progress

A REPORT TO THE COMMITTEE ON SCIENCE AND ASTRONAUTICS

U.S. HOUSE OF REPRESENTATIVES

BY THE NATIONAL ACADEMY OF SCIENCES

1967





**applied science  
and  
technological progress**

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Panel on Applied Science and  
Technological Progress

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## PREFACE

In December 1963, the Committee on Science and Astronautics of the United States House of Representatives concluded a formal agreement with the National Academy of Sciences. The purpose of the agreement, which evolved into the first contract between Congress and the Academy, was the production of comprehensive study and pilot programs designed to highlight some of the critical policy issues which Government must consider in its decisions to support or otherwise foster research in America.

This report is the second submitted to Congress under the agreement. The first, entitled "Basic Research and National Goals", was submitted in March 1965. Neither report has been easy to undertake since each has required the careful evaluation of very complex and elusive relationships—including those which constantly shift between government, science, technology, society and individuals.

In carrying out the terms of the agreement and in developing the form and substance of the report, we in the Congress are particularly indebted to Representative Emilio Q. Daddario who, as chairman of our Subcommittee on Science, Research and Development, served as the congressional agent and focal point throughout, and to Dr. Harvey Brooks who, as Chairman of the Academy's Committee on Science and Public Policy, served in similar fashion on behalf of the Academy.

It is my belief that this report represents not only genuine achievement and utility in itself, but is another significant milestone in Congress' methods of gathering talented, objective assistance to its use.

GEORGE P. MILLER, *Chairman,*  
*Committee on Science and Astronautics*

(v)

# NATIONAL ACADEMY OF SCIENCES

OFFICE OF THE PRESIDENT  
2101 CONSTITUTION AVENUE  
WASHINGTON, D.C. 20418

MAY 25, 1967.

HON. GEORGE P. MILLER,  
*Chairman, Committee on Science and Astronautics, House of Representatives, Washington, D.C.*

DEAR MR. MILLER: In March 1965, I had the pleasure of transmitting to you the report, *Basic Research and National Goals*, prepared in response to a request from your Committee that the National Academy of Sciences look into the special problems of basic research and its support by the Federal Government in the United States. With similar pleasure I transmit to you herewith the report, *Applied Science and Technological Progress*, also prepared at the request of your Committee and in every sense a sequel to the earlier report. The present report deals with the special problems of effective applications of the resources of science to advances in technology.

As in the case of *Basic Research and National Goals*, I assigned responsibility for preparation of the Academy's report to the Committee on Science and Public Policy, now under the chairmanship of Harvey Brooks. As in the earlier study, the Committee selected a distinguished group of highly competent individuals as a study panel to undertake examination of the relevant problems and to prepare a report setting forth their findings and recommendations. This they have done in the form of a collection of individual essays, which are preceded by a brief summary which includes a set of recommendations.

The study panel has not only sought to identify the principal elements of successful applied research leading to new technology of national importance, but also to indicate the characteristics of an environment conducive to enhancement of those elements. In addition to analytical discussion and thoughtful opinion, the papers include many detailed examples of the development of useful products and processes. Also included are proposals for effective application of our resources in science to the most critical social problems facing the Nation.

The Academy is deeply grateful to all the participants in the study. This gratitude extends not only to those who gave so generously of their time and energy directly, but also to the large number who contributed their thoughts and opinions less directly but nonetheless effectively. We earnestly hope that this report will prove useful to those in our Nation who have responsibilities for critical decisions in the matters with which it deals.

Sincerely yours,

FREDERICK SEITZ,  
*President.*

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## INTRODUCTION

Although there is no sharp division between basic and applied science, it is a fair estimate that between 70 and 90 percent of our country's "science" budget goes for applied research and development. The state of applied science in the United States is thus a proper matter for scrutiny by the Congress. On this account the Subcommittee on Science, Research, and Development of the House Committee on Science and Astronautics asked the National Academy of Sciences to assess American applied science: Is it effective? Are we getting our money's worth? Are there ways to improve it? What is the proper role of the Government laboratory, industry, the universities? What should Congress itself do to strengthen our country's capacity to achieve its many goals through the application of science? As was the case with the previous congressional study, *Basic Research and National Goals*, a set of questions covering these matters was referred to the Committee on Science and Public Policy, which in turn invited a group of panelists to consider them.

In the study, *Basic Research and National Goals*, the study panel employed a device that may be called the "criticized essay." Each panelist prepared an essay that dealt with particular aspects of the problem; the essays were then modified in reaction to criticisms offered by other panelists, each panel member, however, taking full responsibility for his essay. The entire collection of essays was summarized in a short introduction. Essentially the same procedure was followed in the preparation of this set of essays.

The present collection of 17 essays thus represents the separate opinions, and experience, of each author or group of authors as to the state of applied science in the United States. In addition, before the study panel was formed, some 50 scientists and engineers throughout the country were asked to submit ideas about relevant issues to be examined in such a study. These ideas have been taken into account in the first essay by Brooks, although the opinions expressed in that essay are those of the author and not necessarily those of the correspondents.

In the search for an effective approach to the task, an attempt was made to analyze the subject into a number of separate topics, each introduced by a series of related questions. It quickly became apparent, however, that applied research is more complex and diverse in its goals, its standards, and its style than is basic research. Applied research can range from pure empiricism to abstract theory, from the highly particular to

the very general, from a personal and individualistic effort to a highly scheduled and organized team effort. At one end of the spectrum, applied research is indistinguishable in its general approach and methodology from the purest type of basic research. At the other end it is best described as enlightened tinkering. Of greater importance, however, is the fact that all the styles along this spectrum may enter at one time or another into a single successful applied research effort. Although applied science today usually requires increasingly sophisticated and theoretical training, it is still far from being completely a monopoly of highly educated specialists.

Because such a large proportion of applied science, both governmental and industrial, is concerned with applications of the physical sciences, there is a tendency to overlook the increasingly important biomedical and social sciences. These areas are represented explicitly by only three essays: the essay by Visscher, which deals with applied science in medicine, the essay by Hendricks dealing with applied research in agriculture, and the essay by Bauer on applied social science. Whereas application of the physical sciences is centered primarily in nonacademic organizations, application of the life sciences takes place to a much larger extent within academic institutions, principally in medical schools and affiliated teaching hospitals and in agricultural experiment stations and extension services associated with universities. Even in the social and behavioral sciences the "practitioners" often have a closer association with the university in professional schools of business and education.

Thus, "technological progress" in the context of this report must be interpreted as embracing more than engineering; it also includes medicine, agriculture, scientific management, and educational psychology. The last is considered, at least implicitly, in the essay of Gerard.

This variety of styles is reflected in the manner in which individual panelists addressed themselves to various aspects of the eight questions finally chosen as epitomizing the problems to which the Congress has addressed itself. Some of the panelists, notably Edward Teller and Harvey Brooks, chose to respond broadly to all the questions; others considered the social implications of applied research; and still others chose to consider the special relevance of specific or broad applied fields. Thus the essays tend to fall into three major groups. Those of Brooks, Teller, Bode, Smith, Kimball, Soderberg, and Charpie are concerned with philosophical, historical, educational, or sociological aspects of applied research or engineering. Those of Bauer, Weinberg, Hutchinson, and Gerard are concerned in a general way with the mobilization of our technological enterprise around urgent social problems. The remainder are concerned more specifically with an assessment of our position and our potentialities in certain kinds of applied science. Thus Cloud and Mc-



Kelvey, as well as Hendricks, deal with predominantly Federal applied research; Gershinowitz, Suits and Bueche, and Goldman and Frey with industrial applied research; and Visscher with medical research. Because the field under study is so very broad, there was relatively little overlap among the panelists. In consequence, essays in this report tend to confront each other rather less sharply than did the essays in *Basic Research and National Goals*. Nevertheless, in spite of the breadth of their coverage, three pervasive points of consensus did emerge from the essays.

Perhaps most strikingly, the essayists either explicitly, or more often implicitly, seemed to conclude that the most important invention in the pursuit of modern (as opposed to older) applied science is the big mission-oriented industrial or Government laboratory. In fact, modern applied science can hardly be discussed without reference to these homes of applicable science. These institutions derive their power from three sources: (1) their interdisciplinarity and the close interaction between basic research and application; (2) their methodology for precipitating and organizing coherent effort around big problems; (3) their ability to adapt their goals to the requirements of their sponsors. The industrial laboratories in particular have been remarkably successful in adapting the requirements for a vital scientific environment to the profit motivations of business enterprise, demonstrating that first-rate science and corporate profitability are mutually supporting rather than incompatible goals. Many of the suggestions for exploitation of science in the solution of applied problems therefore amount to suggestions as to how to maintain the vitality and spirit and sense of mission of these Government or industrial institutions in which most of our applied research is done. Just as *Basic Research and National Goals* had as its primary institutional focus the university (at which most basic research is performed), so this study, possibly less explicitly, has as its primary institutional focus the multidisciplinary mission-oriented laboratory, at which most applied research and development are performed. Again medicine is a partial exception to this generalization, although even here there is likely to be a trend toward separate mission-oriented institutions dedicated to the development of applications and the "systems engineering" of medical care.

The relation between the mission-oriented institution and the university is a recurrent theme in this report just as it was in the earlier report. Because basic research is done in both the research-oriented university and in the mission-oriented institution—particularly the Government laboratory—the question of how limited funds are to be allocated between the two kinds of institutions received considerable attention in *Basic Research and National Goals*. In the present report the matter is looked at from the other direction: a tension between the mission-oriented institution and the research-oriented university arises not so much because both want to perform applied research with a limited Government

pool of resources, but rather because the mission-oriented institution depends for much of its intellectual stimulus and for its staff upon the university; yet the university, with its dedication to basic research, tends to relegate applied research, and those who are dedicated to it, to a secondary position. The situation is somewhat different in the life sciences and in the behavioral sciences, in which applications as well as basic investigations are more centered in academic institutions; nevertheless, the same tension between basic scientists and appliers of science is often apparent within the university between "professional" and "academic" faculties. There is little doubt that this tension exists; yet on the whole the panelists who deal with it take the view that its creative aspects outweigh its disadvantages as long as first-class people move into applied science and a reasonable balance of effort is maintained between the two types of institution. This will tend to remain true as long as the universities remain bastions of basic research, even if most of the money is spent on applied research. The basic research of today is likely to become the applied research of tomorrow, and it is easier to move in that direction than the reverse.

The university, dedicated to basic research, must thus continue to set standards of excellence in relevant scientific disciplines for the mission-oriented institution, and the mission-oriented institution must provide a motivation in practical matters for people who are products of the university. Out of this tension ought to emerge a symbiosis leading to better universities and better mission-oriented laboratories. A continuing flow of people between Government, industry, and educational institutions is one of the greatest sources of strength in American science and technology, and must be encouraged as much as possible.

A second and equally important point of consensus is the recognition, in several of the essays as well as by others during the panel's discussions, of the Government's special responsibility for the integrity and sufficiency of man's environment and for dealing with the social questions that arise from such concerns. Problems of such thrust are likely to transcend all private interests and thus to be matters of national and regional concern, requiring national and regional instruments to deal with them.

The social impact of science and technology was the third area in which the panelists found considerable agreement in discussion, although few deal with it explicitly. This involves not only problems relating to the integrity and sufficiency of the environment but also questions of individual and institutional obsolescence or even irrelevance, and of basic social structure and the means by which it reacts to its changing technology and environment. Another way of saying this is that the major problems that now confront us—Brooks' four P's: population, poverty, peace, and pollution (to which one should add race relations and education)—have strong *social* components. Thus, in advising Congress

concerning how to focus and mobilize the country's scientific and technical resources for the future, one must suggest how a scientific apparatus primarily oriented toward physical technology can be reoriented for purposes of dealing with social and environmental problems. Important elements of this problem include the large, interdisciplinary laboratories of agencies of the Federal Government. Their adaptability and applicability to new national problem areas as their original missions mature is urged. Suggestions in this direction, tentative though they are, are made explicitly in several of the papers. Insofar as technological components of social problems do exist, any suggestions that improve our capacity for better technology also help to reduce some of our social problems; and to this extent all the papers bear, though indirectly, upon the great social problems.

The foregoing remarks do not pretend to do justice to the many threads and currents running through this set of essays. For these one must turn to the essays themselves. Yet we believe, in spite of their wide diversity of viewpoint, that the majority of the panelists do convey the impression that American industrial research has been a magnificent triumph; that governmental research has, on the whole, been well done and in some agencies is outstanding and that, as we gradually shift our structure toward these other, much more difficult, socially determined problems, we have a right to expect many important successes. To assure this, however, we must press forward with ingenuity, with imagination, and with sufficient intensity—but not with so much intensity as to exclude those leisurely gaps during which creative insights may spring from the subconscious depths of the prepared mind.

Although specific answers to the eight questions posed to the study panel do not emerge from these essays, we believe that the essays, the preliminary correspondence, and our discussions do suggest a degree of consensus on each of the questions, which we present here.

*Question 1.* Given a broad national goal such as defense, health, prevention of pollution, environmental forecasting, stimulation of civilian technology, etc., what criteria can or should be used in arriving at a proper balance of support along the spectrum from basic research through applied research to development in support of each goal?

Basic research, applied research, and development in a given broad area represent a coherent interrelated effort, so that the question as posed probably has no single answer. The conventional model of progress from basic research to final development and use is an oversimplification. It is not a simple linear progression, in which the essential scientific content is narrowed down from stage to stage. Many competent industrial laboratories do not characterize basic research and applied research as categorically distinct activities. Often a new discovery or a newly identified

problem will transform research previously regarded as basic into applied research. Even the distinction between applied research and development is often minimized, and in practice all these types of activities must proceed simultaneously in a process of continuing mutual interaction. Goal-directed research is actually indivisible.

On a national scale there should be enough basic research to generate a pool of skilled people who can be drawn into applied research and technology when occasion requires. But the actual proportional allocation of investment in basic research, applied research, and development will vary from field to field and with time and the state of the art. All advanced nations of the world perform about the same amounts of scientific research relative to their gross national products, but they vary significantly in the relative sizes of their investments in technology. Typically a science-based industry spends about 5 percent of its research and development investment on basic research, and about 10 to 15 percent on research as a whole.

*Question 2.* How can we arrive at a judgment as to what applied research should be supported for general development of the state of the art without being tied to specific projects and their associated time schedules, and what applied research will be more effective if tied to specific projects and fitted into a highly structured plan?

The most effective applied research is usually done in an environment in which general goals are reasonably clear, but under constant reassessment in the light of new research and analysis. On the other hand, serendipity may be just as important in applied research as in basic research, and applied research results often become useful for purposes other than those originally intended. Successful research organizations have sufficient flexibility and internal freedom to take advantage of new opportunities to the full. In connection with even the most focused effort, it is important that some research be done that looks beyond the time-bound limits of the particular system under development. Applied research that takes off from an original system need, but that may extrapolate considerably beyond it, is often more successful than applied research undertaken in the abstract. Also, close interaction between systems engineering and applied research can be a key factor in success. Each reinforces the other. Some of the people engaged in applied research should constantly be attempting to generalize their results beyond immediate system needs; for this reason it is frequently important that they approach their tasks with the intellectual techniques of basic research. Ideally, in almost any technological project, a substantial fraction of the research component should look beyond the objectives of the particular project with which it is associated.

*Question 3.* What is the best kind of training for the applied scientist, and what kinds of research support are particularly relevant

to the training of applied scientists in universities? What kinds of training and experience produce the most effective teachers of applied science or engineering? How can Federal support of research and training be channeled in such a way as to encourage the right kinds of faculty development in the applied sciences (engineering, medicine, agriculture, scientific management)?

A good applied scientist should first of all be a good scientist by standards similar to those applied to basic scientists. However, a primary difference between basic and applied scientists lies in their respective attitudes toward disciplinary specialization and personal recognition by professional peers. Good applied science usually, though not always, requires greater breadth and a more eclectic attitude toward knowledge. A recurring shortcoming of university training in applied science and engineering arises from disciplinary over-specialization. The applied scientist must often be satisfied with just enough understanding for his immediate purposes and cannot pursue every interesting lead to its logical conclusion. He must be interested in more than strictly intellectual solutions.

Good applied science must usually relate to the mission of the institution or organization in which it is done. This makes it difficult to conduct good applied research in universities unless it relates to a mission that belongs to the university itself. There are some university missions, such as the educational process itself, the delivery of medical care in teaching hospitals, or the development of research instrumentation, on which good applied research can be done, and these are probably the most suitable for training applied scientists or engineers in universities. The advent of the computer makes possible simulation of real-life systems at little enough expense to be suitable for university work, and provides opportunities for greater university involvement in real-world problems. Close association between universities and external mission-oriented research institutions or science-based industries is of importance, both for exposing students and faculty to the challenge and opportunity in applied research, and for accelerating the flow of ideas between basic research and applications. This should be considered, for example, when organizing and locating new federally financed laboratories.

*Question 4.* Is the United States neglecting applied science as compared with both basic science and engineering development? What steps should we take as a nation to attract better minds into applied science? What criteria can the layman use to appraise our national performance in applied science?

The study panel failed to come to full agreement on this question. Some feel strongly that applied science is being neglected, others that the quality of people and leadership being attracted into applied research is not commensurate with expenditures. Still others feel that the normal migration of people in the technical manpower market as the supply of

scientists and engineers grows would compensate any temporary deficiencies. There was some feeling that, despite the success of affiliated teaching hospitals, there was a lack of adequate institutional arrangements for research and development on the system of delivery of medical care as compared with the study of disease problems and techniques of treatment. Increasing influence of university mores in American society may be the cause of a tendency to regard applied science as a second-class activity.

Applied science probably needs more heroes. Even in applied work there is a tendency to give all the credit to the man who had the original idea and overlook the exhausting work and single-minded dedication necessary for successful implementation. While association of students with mission-oriented laboratories can be beneficial if it brings them in contact with real applied-science heroes, close contact with low-quality applied laboratories or applied scientists can also drive ambitious students away from applied work altogether.

The group identified several specific areas, such as manufacturing processes, materials fabrication, and the study and evaluation of environmental effects of industrializing society, in which there is room for more scientifically oriented applied research.

It is difficult to establish criteria for the over-all success of the U.S. performance in applied research. International comparisons probably form one basis for judgment. The so-called "technological balance of payments" is probably another measure, but is so complicated by economic and market factors that its true meaning is uncertain. Other criteria include the relative growth rates of science-based industries, and the success of Federal programs in generating technology that is picked up and developed in the private sector. There is need for the development of various kinds of statistical indicators for evaluating success of applied research in dealing with public-sector problems, e.g., health statistics and pollution indices.

*Question 5.* Industrial or Federal laboratories, how can we appraise the effectiveness of the laboratory in the performance of its missions? How can the laboratory audit its own performance, and what criteria can the management use to appraise progress on individual projects? At what level of detail should applied research be directed from above? How are applied researchers best motivated in such laboratories?

Part of the job of every Federal laboratory and program office should be to develop its own self-evaluation criteria, having first formulated its mission in terms its key people understand and accept. Each major Federal laboratory or contract research center should conduct systematic retrospective studies of its own performance, but privately and internally. On the other hand, such studies, while useful, should be interpreted with

caution, especially in relation to basic research. Both internal audit committees and external visiting committees are valuable tools of self-appraisal.

The best atmosphere for applied laboratories is characterized by internal freedom under strong leadership. Success in applied research is seldom achieved by authoritarian methods, e.g., by directives from the head. Consensus must be developed at many levels by discussion and argument conducted in an atmosphere of mutual intellectual respect. Each level must have considerable freedom in the use of the resources allocated to it. Multiple forms or layers of administrative control are especially stultifying. Lines of scientific communication must be as short as possible and not necessarily congruent with administrative organization.

*Question 6.* What are some of the most significant achievements of applied science worldwide in the last ten years? What were the respective roles of basic and applied research in the achievement of these successes? At which point was the possibility of the final goal first perceived, and how was the effort organized toward achieving it?

The study panel did not attempt to prepare any systematic catalogue of achievements. Examples include agriculture, computers, nuclear power, solid-state electronics, synthetic fibers, high-speed aircraft, antibiotics, and satellite communications. In all these fields the United States has been the leader, although a few participants in the study felt that secrecy in the Soviet Union may have encouraged us to unjustified optimism in this respect. In particular, these individuals felt that the United States was possibly lagging in meteorology, in oceanography, and in some aspects of nuclear energy and space technology. It is worth noting that even when the first idea originated elsewhere, the United States has often been able to make an earlier large-scale or commercial application. This is largely due to our unmatched pool of technical manpower, working in unusually flexible and adaptable technical institutions. Our best organizations, especially in industry, have the ability to adapt their structures to the needs of particular technologies without being inhibited by management or political preconceptions.

The case histories studied by the panel underline the great diversity in patterns of success. New ideas may come from almost anywhere: from a basic scientific discovery, from recognition of a market need, from a management decision, from an obscure inventor, from the technical literature, or from a casual conversation at a professional meeting. A key factor in successful applied research is the achievement of voluntary cooperative effort among scientists and technologists of diverse backgrounds—the association of people who become sold on a new idea. Technical leadership is also very important. The technical leader is

the individual who matches the world of science to the world of society, with a foot in management and a foot in science. The perception of goals often changes as research proceeds; many key technical events occur after the first establishment of objectives, and change the objectives. Forward coupling to the next stage, including the ultimate user, is important all along the line. A successful pattern of technology transfer often involves people moving with ideas from research all the way through production, and organization should make this easy. It is very difficult to transplant new ideas from one organization to another. The development of new ideas should be left in the hands of the originating group until sufficient probability of success has been demonstrated. New ideas should not be transferred prematurely just because they lie outside the assigned tasks of the originating organization.

*Question 7.* Should the United States strive to maintain a very broad spectrum of capabilities in applied science, or should it concentrate on fewer goals and pursue these with maximum possible effort?

While the study panel did not address this issue directly, preference for the broad-spectrum approach seemed to be implicit in the discussion. One reason for this is the increasing interdependence of advances in different areas of science and technology. Many important technological developments result from confluence of apparently unrelated streams of science or technology. On the other hand, there can be too great diffuseness. Selectivity must increase rapidly as one progresses from the research to the development end of the spectrum, but, as stated earlier, applied research in parallel should continue to extrapolate beyond immediate system needs. Enthusiasts for particular development programs sometimes argue that the tightest possible time schedule is actually the most economical in terms of total dollars spent. This is true only if the whole organization—people and facilities—is already in being. This contention overlooks the importance of new knowledge and understanding acquired in the course of development, which has been strongly emphasized in the recent report on Project Hindsight, and also the importance of sheer inventiveness of the inspirational idea resulting from living with a problem continuously. The panel believes that most engineering developments can be pursued more economically through a step-by-step approach, with careful verification of critical points, and scale-up of organizational and facility commitments by stages. The crash program is justified only by time-urgency of the final result, not by economy or efficiency in execution.

*Question 8.* What criteria can be used to decide whether a new technology is "ripe" for exploitation on a large scale? What methods are most effective for appraising the state of the art to determine the



feasibility and timeliness of a major technological effort? What kinds of questions should the layman ask of the experts so as best to form his own independent judgment of the ripeness of a new technology, especially if the experts disagree?

The basic technique is to identify the key technical problems, then appraise the state of the art and the likelihood of solution. The key technical problems are those on whose successful solution the success or performance of the ultimate system will stand or fall. Problem identification in this sense is best carried out in a systems-engineering environment, where there is intimate and continuing interaction between systems people and research people. Systems engineering should be distinguished from systems management on the one hand and operations research on the other. It is a staff or appraisal function, not a management function, and it involves an intimate relationship between research and engineering.

One should not, however, discount the importance of key individuals or groups dedicated to the advancement of particular ideas or technologies. While their enthusiasm must be somewhat discounted, the mere existence of such commitment may be as important for ultimate success as the more objective technical criteria, given the uncertainties of such technical judgments before the fact. Congress should not attempt to second-guess the experts on technical appraisals, but it does have the responsibility to convince itself that the experts have asked themselves the right questions, especially concerning bottleneck problems. It is also important to be aware of certain common biases. For example, technologists already committed to a particular line of effort tend to be oversanguine, to minimize difficulties and underestimate costs. On the other hand, scientists often tend to be overconservative about technological developments and to call for more research. Often they underestimate the applicability of the science that they themselves have developed. There is a universal tendency to be over-optimistic about technical progress in the short run, but too conservative about the longer-range future. There are characteristic cycles of optimism and pessimism in the progress of any technical project, and they often tend to lag actual progress. In general, too rapid scale-up of effort is likely to run into trouble even when specific difficulties are not apparent at the outset. Again, one must underline the importance of carrying on small-scale parallel applied research or oriented basic research as insurance against unforeseen problems or technical surprises.

In appraising the situation, it is important for Congress to listen to the skeptics as well as the enthusiasts, and to ask the enthusiasts to answer the arguments of the skeptics. Laymen can learn a great deal from the confrontation of experts even when they do not understand the details. Especially in applied science and technology, priorities and goals can be

established only through a multidimensional interaction between scientists, technologists, public servants, and the general public.

What, then, can we conclude from this exercise, and what can we recommend to Congress? Again, recognizing that our intent was not to produce a consensus, we nevertheless were able to formulate some specific conclusions and recommendations that would probably be agreed to by most of the panelists. After these views were formulated, we could see that they fell logically into four groups dealing generally with (1) the nature and strategy of applied research, (2) the environment and institutions in which applied research is carried out, (3) the individual scientist, and (4) the role of the Federal Government. There is some overlapping in statements in the Conclusions and Recommendations, which are arranged under these four main rubrics, but we find this interconnection preferable to an arrangement that excludes it.

## CONCLUSIONS AND RECOMMENDATIONS

### Group I. With Respect to the Nature and Strategy of Applied Research

#### 1. *The interaction between science and technology is complex*

In examining examples of the successful translation of science into technology, one is struck by the diversity of successful patterns and organizational structures. There are no simple formulas for success, and for this reason success is most likely when laboratory management has wide latitude in adapting and restructuring the organization to suit the particular problem areas or technologies with which it is currently dealing. Despite this need for administrative flexibility, it is possible to identify certain characteristics of the research environment that facilitate transfer of new scientific results to useful applications and fruitful feedback from applications to science. They include the following:

a. The key individuals in the research organization are fully aware of and sympathetic to the principal goals of the organization, but at the same time the research mission is defined in broad-enough terms so that it retains its validity as circumstances and the state of technology change.

b. The organization is willing to consider and implement new ideas or initiatives on their own merits regardless of the organizational level or functional subdivision in which they originate, or even if they come from a source outside the organization.

c. People within the organization are receptive to moving between the more fundamental and the more applied activities, and also to changing specialties or scientific disciplines. The artificial barriers that sometimes exist between disciplines and between fundamental and applied work are minimized.

d. The organization has a quick response in recognizing and funding new ideas, at least up to the point where the feasibility and desirability of a larger commitment can be assessed.

e. At each organizational level the individual responsible has some freedom in redeploying the resources at his disposal without extensive review by higher authority. This is, of course, more true in research than in development, where the necessity of fitting into a system plan is important.

f. There is full communication through all stages of the research and development process from original research to ultimate application. A good deal of overlapping activity between each of the stages is present.

g. The system of reward and recognition emphasizes technical contributions to the goals of the organization, if necessary in preference to proper organizational behavior.

**2. *A broad spectrum of scientific disciplines and technical skills is required***

To an increasing degree the advance of technology requires contributions from a variety of scientific and technical fields. This fact imposes the necessity not only for effective communication within the research organization itself but also interaction with contemporary work in science and technology outside. The purpose of an effective research organization is to ensure the maximum possible opportunity for matching "problems" to "solutions"—the latter possibly coming from apparently little-related fields. The pattern of technical communications will not always, or even usually, reflect the organization chart.

**3. *The transfer of technology from the laboratory to a producing or operating organization which builds, sells, or uses it, is a vital and often underestimated step in technological innovation***

In industry the using component may be a manufacturing organization, but often involves the final customer as well. In Government the using component may be an operating organization such as a military service, or a service agency such as the Weather Bureau. Often the user, as in agriculture or public health or physical standards, is in both the private and the public sector. The transition from developed technology to usable product, service, or public action is a well-known problem area, in which promising developments often falter or even founder. Effective transfer of each technical advance tends to be a unique problem, requiring a special approach tailored to the specific characteristics of both the research component and the using components or customers. The transfer process requires explicit attention very early in the development process as soon as some probability of *technical* success becomes apparent. It is well established that the best way to transfer some new types of technology is through the movement of knowledgeable people, either temporarily or permanently. It may be necessary, for example, for the originator of an idea to himself follow his brainchild into development, testing, or final production or utilization. Or, alternatively, operating people may have to be brought into the laboratory temporarily to learn the new technology early and to influence its development from the user standpoint.

**4. *Goals in applied research are reached only by reducing them to a series of researchable, relevant components, but this is a dynamic process, subject to continual review as new results emerge***

In establishing a viable program of applied research it is not sufficient to identify broad problems such as pollution or urban design. Rather it is essential within such broadly stated problem areas to identify the specific questions to which it appears feasible to obtain meaningful answers. The determination of technical approaches must be done by technical people, and the input of technical information to the decision process should come as directly as possible from the people who are actually doing the work, regardless of their position in the organization. Technical information filtered through multiple levels of reporting to the top tends to lose its integrity, often resulting in poor decisions.

On the other hand, the setting of research goals and priorities especially in the public sector is a task that requires a many-sided interaction between scientists, technologists, public officials, and informed public opinion. Such goals can neither be imposed by society unilaterally on the scientists nor determined by technical considerations alone. In the mobilization of technical effort around significant problems of society, it must be borne in mind that the highest-quality technical people cannot be drawn into projects solely on the basis of the alleged social importance of the problems. They must also be able to see an opportunity for making concrete, identifiable contributions that relate to their particular talents and background.

**Group II. With Respect to the Environment and Institutions of Applied Research**

**1. *Successful and relevant applied research is most often carried out by coherent institutions***

The setting of sensible and realistic goals for applied research projects is usually done most successfully in close interaction with systems engineering or analysis. Systems studies that take into account all phases of a problem, and research aimed at exploring feasibility, must interact and reinforce each other. In many successful technical developments, there is a continuing three-way interaction between the changing concept of a system, the development of its components, and the evolving understanding of the environment in which it is to operate. This is true not only for engineering systems, but also for "systems" such as medical care, education, or transportation, in which physical components interact with social environments. The implication of the above considerations is that the guidance of applied research, unlike that of basic research, requires an institutional environment that is capable of dealing with all facets of a problem in a coherent fashion, developing a "coherent doctrine" into which the various

subsidiary research goals can be fitted in a logical fashion. An applied research program administered by a headquarters staff distributing small sub-tasks among many participants is seldom as effective as a laboratory that assumes full responsibility for a major problem area.

**2. *Communications barriers imposed by security or other requirements reduce the productivity of applied research***

Applied research, like basic research, thrives in an atmosphere of free exchange of ideas and maximum exposure to scientific criticism. While security restrictions or compartmentalization for proprietary reasons are sometimes necessary, it is important to recognize that they exact a substantial price in loss of effectiveness in the research effort. Too often security or "need-to-know" restrictions serve only to protect inadequate or ineffective work from critical scrutiny by a larger technical community. Such restrictions should be imposed only for the most urgent reasons, and should be constantly reviewed to determine whether the gains are worth the cost.

**3. *Applied research organizations should interact with universities wherever possible***

In planning for the creation or development of mission-oriented research institutions, consideration should be given to facilitating interaction with universities. For Government-owned laboratories, interaction with industry in similar lines of work is also important. The mission-oriented laboratory benefits from new ideas and viewpoints brought in by outside scientists, while the university community is stimulated by contact with problems of application, which often trigger new lines of fundamental research. Applied research and development people should have opportunities to teach and to come into contact with graduate students, recognizing that any temporary loss of productivity will be more than compensated by the long-term benefits of the exposure of able students to the intellectual challenge of applied research.

**Group III. With Respect to the Individuals Who Conduct Applied Research**

**1. *Applied research requires personal mobility***

Since the transfer of technology and the applications of new science frequently occur through the movement of people among different kinds of scientific activities and institutions, policies for the support of research should always be considered in light of the need to foster such mobility and to reduce organizational and intellectual barriers between basic and applied research, between Government, universities, and industry, and between different fields of science. Many foreign observers cite the mobility of scientists among institutions and disciplines in the United

States as a major factor in our superior performance in the application of science. It is important that in our desire to protect the integrity and objectivity of basic science we do not discourage this important type of mobility.

## ***2. The university plays a crucial role in the education of people for applied research***

The highest-quality applied work is often done in an environment in which a substantial pool of people with original training in basic sciences can be drawn upon for applied research and development activities, especially as these people broaden and mature in experience. Thus it is desirable and natural that the educational system should produce an excess of people trained at the research frontiers of science and engineering over those required to feed back into the educational system itself. Not every student trained in basic research should expect to have a long-term career in fundamental science. Too frequently, however, the general climate in universities and professional societies tends to denigrate the intellectual challenge and satisfaction of applied research; in discovering these things for himself, a scientist or even a research-trained engineer must often overcome adverse attitudes fostered by his graduate studies and professional associations. A greater effort should be made to create an atmosphere in graduate education that would anticipate and encourage the subsequent entry of many students into applied work. Although this problem is less acute in the professional schools—especially in medicine—it is not unknown even in this area.

## ***3. The technical entrepreneur is frequently the catalyst of progress***

The technical entrepreneur, or missionary—the man who carries the torch for a new idea—is often the catalyst of technical progress. Even though he may sometimes be more distinguished for enthusiasm and ingenuity than for profound technical understanding, his courage and tenacity are frequently vital elements of successful innovation. We need to identify such individuals early in their careers, to encourage appropriate educational preparation, and to ensure an occupational environment that will enhance their contributions.

It must be recognized, however, that many successful innovations have been accomplished without such zealots. Some very able and original technical people, who have contributed important innovations, are not especially vocal or persuasive. Infectious enthusiasm may impart courage when—as is frequently the case—courage is needed; but enthusiasm will not, of course, repeal a law of nature, if that is the roadblock that stands in the way of a successful innovation. The technical idea that has glamor or popular appeal or is easily explained and dramatized is not always the best idea, or the one most likely to lead to successful application in the long run.

## Group IV. With Respect to the Role of the Federal Government in Applied Research

### 1. *Applied research establishments of the Federal Government should be examined for redeployment in the light of changing national needs*

The large interdisciplinary applied research establishments of agencies of the Federal Government comprise an important national resource. Although many of these laboratories were founded upon a specialized scientific or technical field with major mission orientation, by virtue of their scientific breadth they now have impressive and versatile capabilities. In addition to their fields of origin, they undoubtedly could perform with great effectiveness in a variety of contemporary scientific fields, some within the purviews of Government agencies other than their original parent organizations. The programs and organizational locations of Federal laboratories should therefore be examined at appropriate intervals to determine whether the maturity of their original missions would justify some reassignment of effort to emerging problems of challenging national interest. New national missions or regrouping of old missions often become possible as a result within science and technology themselves. Thus redefinition of agency purposes is essential for exploiting such developments in a timely and effective way and realizing the maximum benefits from prior investments in science.

### 2. *Patents play a vital role in utilizing the results of applied research*

In modern industry, in which useful economic innovation is sought as a source of growth, patent protection is almost always essential to successful useful application. The originator of a new process, product, or service generally cannot risk the very considerable investment in production equipment or in exploring and developing a market if his competitor is free to copy his invention and use it without paying for the development. A very large fraction of the industrial applied research now in progress in this country would not be justified in the absence of potential patent protection, or would have to be accomplished under tight industrial secrecy. The widespread practice of promptly publishing scientific and technical results originating in industry could not exist without patent protection. Patents are thus the antidote for both the pirating of ideas and the maintenance of secrecy.

### 3. *Studies of the history and sociology of applied science are important*

Studies of the history and sociology of science and technology should be encouraged, in order to further understanding of the principles behind the great variety of successful patterns of applied research and its transfer. To be effective, such studies probably require the participation of both natural scientists or technologists and social scientists or



historians. Both academic studies and retrospective self-studies within research organizations are desirable, but the complexity of the subject must be recognized and premature generalizations and policy application avoided.

#### ***4. When possible, forecast technological progress***

The effort at technological forecasting—that is, projecting future technological possibilities and probabilities in relation to current knowledge and research and in relation to potential interactions with society, with the economy, and with the natural environment—can provide important guides to the identification of new goals for applied research, and in educating and alerting scientists and technologists to new possibilities. Technological forecasting is, however, a relatively new field and its methods are subject to further improvement. It can do more harm than good for research planning if its results are treated as more than rough first approximations. Part of the purpose of research is to keep many future options open to society, and the purpose of technological forecasting is primarily to identify and expand these options rather than to foreclose them.

#### ***5. Concern with the environment must be a growing Federal responsibility***

Understanding, prediction, and control of the consequences of technology, industrialization, and urbanization on man's physical and biological environment are urgent. A broader and more intensive national effort is needed on the integrity and sufficiency of the environment. This effort must also encompass the logical constraints that are placed on the quantity and possible quality of life, and hence on wise social development, by the availability and quality of natural resources.

## **APPLIED RESEARCH DEFINITIONS, CONCEPTS, THEMES**

*by* HARVEY BROOKS

### **Introduction**

This paper is partly a digest of ideas turned up by correspondents and developed at a preliminary meeting on April 24, 1966, and partly interpretation based on my own experience and observations. It is in ten sections as follows:

1. An interpretation of the distinctions between basic and applied research and a description of their interactions, emphasizing the continuity and indivisibility of the research and development process.

2. Some of the problems and difficulties in studying research as a process, noting especially the evolving nature of the relations between science and technology, which makes conclusions from historical studies of technology of limited relevance to current policy.

3. The relative roles of Government, industry, and universities in applied research, including a fairly detailed discussion of the historical role of universities in applied research in the life sciences and of criteria for university involvement in applied problems.

4. The contemporary interaction between science and technology, stressing the growing role of technology in pure science and the feedback between pure science tools and industrial development, also the increasing trend toward conceptualization in technology.

5. The use of the attitudes and methods of fundamental research in dealing with applied problems.

6. Political and administrative decision-making about technology, the relative roles of the expert and the generalist, and the appropriate degree of societal direction of applied research.

7. Discussion of the university as the characteristic institution of basic science and the mission-oriented, multidisciplinary research institute—industry, Government, or independent—as the characteristic institution of applied science. The concept of a “mission” is defined, and the attributes of successful mission-oriented laboratories are itemized and discussed.

8. The question of the status of applied research and applied scientists in the United States with a tentative sociological analysis of the “snobbery” that exists between pure and applied science. It is suggested that

vigorous national pursuit of equality of educational opportunity may be one of the surest methods of producing more and better applied scientists, since these often come from "upward mobile" segments of the population.

9. The problem of assessing quality in applied research, suggesting that better documentation of applied science would have a positive influence on quality.

10. The seminal role of the technical entrepreneur in spearheading technological innovation.

## 1. Definitions and Concepts

In institutions whose missions include the application of research results to products or operations, the categorization of research into basic and applied is not too meaningful, and has little operational value. Industrial and Government researchers feel particularly strongly on this point because, from the standpoint of research management, the basic-applied dichotomy tends to focus attention on the wrong issues. In fact, all research in a "mission-oriented" organization contributes or should contribute, however remotely in time, to the general objectives of the organization. On the other hand, there is clearly a spectrum of activities ranging from pure research on the one hand to technological development on the other, and to some extent one can locate research activities within this spectrum according to their "appliedness." This relates to two factors, the time scale on which the research is likely to find an application, and the specificity with which the domain of application can be foreseen or the work committed at the time the research is undertaken. The shorter the time horizon and the more evident the area of potential application, the more "applied" the research. Furthermore, there can be a perfectly viable difference in viewpoint between the research worker and his sponsor. Research that may be viewed as quite fundamental by the performing scientist may be seen as definitely applied and may fit into a coherent pattern of related work from the standpoint of the sponsoring organization or agency. The scientist may see his own work in a matrix of interconnections entirely different from that in which the sponsor sees it. Furthermore, in a well-coordinated group of scientists, success in a particular line of applied research may greatly expand the possibilities for basic research. For example, when a new area of development opens up, an important benefit of intensified basic research related to this area is the indirect one of maintenance of technical standards, and the introduction or perfection of new intellectual and experimental tools that might not otherwise be justified. It is not necessary to control the direction of the efforts of the individual research man in order to realize these benefits.

One may discern in the literature two views of the processes of invention and innovation. One is the completely rational view, characterized by such terms as the "management of research" and "the organization of invention" or "cost-benefit analysis." It views innovation as a process that can be completely planned and that is designed to "convert the essential resources of human talents, physical facilities, money, and knowledge into marketable goods and services" (1). The other view, while conceding the need for planning and foresight, stresses the non-rational and fortuitous elements in the innovation process, the fact that "once a process of technical development has begun, it does not usually move in a straight line, according to plan, but makes unexpected twists and turns." It lays emphasis on the "unexpected boundaries of need and technology," on the fact that "invention" often "consists in carrying techniques in modified form from one field to another" and that "we cannot expect answers only from technologies traditionally associated with a problem" (3). While either the rational or the non-rational views of innovation are caricatures when expressed in pure form, my own belief and experience lean toward the second view. This point of view, which seemed to be generally emphasized by our correspondents in this study, will be stressed throughout this paper. It is a view that runs definitely contrary to what I consider to be the current overemphasis on the rational elements of planning and programming of technical work, especially within Government.

Research is best regarded as a continuing process involving a series of contingent choices by the researcher. Each time he decides between alternative courses of action, the factors that influence his choice determine the degree to which the research is basic or applied. If each choice is influenced almost entirely by the conceptual structure of the subject rather than by the ultimate utility of the results, then the research is generally said to be basic or fundamental, even though the general subject may relate to possible applications and may be funded with this in mind. The fact that research is basic does not mean that the results lack utility, but only that utility is not the primary factor in the choice of direction for each successive step. The general field in which a scientist chooses or is assigned to work may be influenced by possible or probable applicability, even though the detailed choices of direction may be governed wholly by internal scientific criteria. Research of this type is sometimes referred to as "oriented basic research." Much biomedical research is of this character, since almost any new knowledge in the life sciences has a fairly high probability of being applicable.

As another example, once the transistor was discovered, and germanium became technologically important, almost any research on the properties of Group IV semiconducting materials could be considered to be potentially applicable, and this has indeed proved to be the case in

practice. On the other hand, research into the theory of zone-refining single crystals (see paper by Goldman and Frey) was of such obvious immediate application to the control of transistor materials that it could be legitimately called applied rather than merely applicable. Prior to the discovery of the transistor, both of these types of research would have been of equal interest and importance from the scientific viewpoint, but they would have been classified as quite fundamental or "pure." Indeed the same two types of research carried out in a university might be regarded as fairly "pure," while in the Bell Laboratories they would be regarded as "applied" simply because potential customers for the research results existed in the immediate environment. Furthermore, the detailed choices of successive steps in the research would probably be different in the two environments, and might lead ultimately to somewhat different investigations even though they started from the same point. If the next step is toward the particular, the research is more likely to be applied, but if it is toward the general, or toward widening the scope of applicability of a technique or principle, it is more likely to be basic. Thus, in the zone-refining example, the research man in a semiconductor industry might concentrate his attention on study of the purification of promising semiconducting materials, while the university scientist might become interested in exploring a wide variety of materials, both in order to study the dependence of purification efficiency on a wide range of material parameters and to explore a variety of materials of very high purity with a view to determining the sensitivity of various physical properties to purity or crystal perfection.

The essential point is that the categorization of research depends on the existing situation in technology and also on the environment in which it is conducted. As definite categories, basic and applied tend to be meaningless, but as positions on a scale within a given environment they probably do have some significance.

Although basic or fundamental research tends on the average to be less applicable in the sense defined above, the terms basic and applied are, in another sense, not opposites. Work directed toward applied goals can be highly fundamental in character in that it has an important impact on the conceptual structure or outlook of a field. Moreover, the fact that research is of such a nature that it can be applied does not mean that it is not also basic. Almost all Pasteur's work, from the fermentation of beet sugar and the disease of silkworms to the anthrax disease of sheep and the cure of rabies, was on quite practical problems; yet it led to the formulation of new biological principles and the destruction of false ones, which revolutionized the conceptual structure of biology. As another example, studies of semiconductor devices have opened up whole new areas of basic solid-state research that would probably never have been con-

ceived of if the problem or phenomenon hadn't first showed up in a practical device. A good example is the tunnel diode (discussed in the paper by Suits and Bueche). It was discovered that the current-voltage characteristics of these devices showed peculiar kinks that could be explained in terms of the atomic vibrations of the crystal lattice, and this in turn opened up a whole new technique for the precise study of atomic vibrations in crystals (3). Similarly, the availability of very pure single-crystal semiconductor materials, which was made economically justifiable only because of their commercial importance, enabled researchers to study the physical and electrical properties of these materials with a precision of detail hitherto impossible. This in turn led to deeper understanding of the theory of the behavior of electrons in crystals quite generally, and really opened up an important new area of theoretical research.

Despite this basic-applied feedback in research, if the criterion used is that of the individual choices of the investigator *after* his initial choice of general field of work, then I think a fairly meaningful distinction can be made between basic and applied research.

Industrial researchers are most skeptical of such questions as "What proportion of our research should be basic and what proportion applied?" or indeed, "What should be the proportions between research and development?" They would rather argue from the point of view of business objectives: research, development, production, and marketing are part of a continuous process of two-way information flow, and any distinctions that tend to place barriers at particular stages in this process also tend to reduce the effectiveness of all its individual components. On the other hand, if the researcher at the most basic end of the spectrum is continually having to change the direction of his efforts at the behest of market and production needs, his effectiveness is largely destroyed. Thus science, to be effective in the whole process, needs both isolation and communication. The research and development process may be thought of as a long chain, the two ends of which are well separated but nevertheless connected firmly through the intervening length. The man at the application end of the chain must be able to obtain information directly from the scientist, but the feedback along the chain to the scientist must not be so strong as to interfere with the conceptual integrity of what he does.

Some classification of research into basic and applied is probably needed to protect some kinds of research activity from unrealizable expectations. Basic research is that which may take the longest time to come to a utilizable fruition, and must be judged by the scientific criteria of conceptual significance and generality. From applied research one expects a shorter time of payoff but does not necessarily demand generality or high intrinsic scientific interest. The best science is often that which has practical and scientific importance at the same time, but this is rela-

tively rare and cannot be the normal expectation. Much of Irving Langmuir's work had these characteristics. His discovery and working out of the properties of atomic hydrogen, for example, was of high scientific importance, but also led to the development of a radical new method of welding. His work on surface films opened up a whole new area of fundamental science, whose existence was barely suspected, and earned him a Nobel prize, but also found immediate applications.

Weisskopf has drawn an interesting distinction between "intensive" and "extensive" research (4). By intensive research he means research aimed at discovering new fundamental laws and formulating new theories of nature. It is usually characterized by very intensive study of a few simplified systems, chosen because they are believed to exhibit the laws or principles of interest in their most generic or easily isolatable form. High-energy physics and modern molecular biology are both examples of areas of intensive research, in which scientists seek to ask and answer a very small number of very fundamental questions. On the other hand, extensive research usually deals with a larger number of questions, which are less fundamental. It aims at elucidating the applicability of fairly well-understood principles and theories to an increasing variety of systems, often of increasing complexity. Extensive research has the characteristic that when a new experimental discovery is made its theoretical explanation is usually found very quickly. Most of chemistry, solid-state physics, and systematic biology are examples of extensive research areas, in which the fundamental principles are understood and the task of research is to discover precisely how they apply to real objects or systems. Extensive research is more likely to be related to applications than is intensive research, and it is no accident, therefore, that most of even the most basic industrial research is of the extensive variety. Indeed, in extensive research the possible ramifications of the underlying principles are usually so diverse and varied that considerations of possible applicability are almost necessary to assist in the selection of problems and research directions. The intensive study of the properties of the Group IV and Group III-V semiconductors in the last ten years was not motivated solely by intellectual curiosity, but was also related to their potential or actual technological interest, as well as to their availability.

On the other hand, the distinction between extensive and intensive research is not absolute. Extensive research can be the basis of great new generalizations or theories. A classic example is Darwin's development of the theory of the origin of species by natural selection, which took place through the accumulation of hundreds of detailed observations representing both applications and tests of the new theory.

Although scientists like to emphasize that fundamental research is "free," it is actually, in another sense, a highly disciplined activity. The discipline is provided by the scientific community to which the researcher

is related. His choice of problem and direction is heavily conditioned by the social sanctions of this community, the requirements of originality, and scrupulous reference to related and contributing work of others. The scientist takes these external constraints so much for granted that he does not consciously view them as constraints, but his description of his own activities as "free" may be quite misleading to the layman, who takes the description unquestioningly. In applied research the individual is subject to somewhat different constraints, but not necessarily more severe. They are a variable mixture of constraints arising out of science and constraints arising from the institutional environment in which the research is done. Although scientists are strongly self-motivated, they are also sensitive to their audience. The audience of the academic scientist is the worldwide community of his professional colleagues or peers in his own specialty, communicating through the official scientific literature, through scientific meetings, through "invisible colleges" of preprint circulation and correspondence, and through personal contact. To the scientist in a mission-oriented organization, his audience is mixed. It consists partly of his professional community, but also to a great extent of the colleagues and superiors within his own organization. To feel successful he must feel appreciated, and this appreciation is never accorded or felt purely in terms of his contributions to science, no matter how excellent they may be. He responds to subtle clues in his organizational environment that indicate his value to the organization. Thus one finds examples of good scientists who have left industry, not because they didn't have complete freedom to follow their own curiosity, but because they felt their work was not coupled to the organization, regardless of how much it might be recognized outside. Conversely, applied scientists in an academic environment often feel unappreciated, not because of any explicit pressure, but simply because of the general atmosphere, and because of the lack of means for dissemination or utilization of results of their work.

## 2. Research on Research

There is increasing concern with the need for better understanding of the research process itself. Several of our correspondents have deplored the lack of systematic scholarship on the research and development process—of research about research. Recently there has been an upsurge of interest in this area, but there is still an absence of solid generalizations based on reliable empirical studies. Much knowledge of the research process comes either from the observations of social scientists with minimal knowledge of the substance of the research area they are investigating, or from the anecdotal evidence of scientists and technologists having little appreciation of the standards of historical evidence and often inadequate appreciation of the economic, social, and cultural factors that influence



the rate of adoption and application of research results. There is a need for greater involvement of scientists and technologists themselves in the introspective study of the research process, but subject to the critical scrutiny of social scientists or historians. Many scientists and engineers tend to be unwilling to search for consistent patterns of success in research because they realize the importance of fortuitous interconnections and intellectual spontaneity, and they worry lest dissection of the research process squeeze out this spontaneous element and destroy the environment of successful applied research through premature policy application of untested or overgeneralized findings. The very fact that the natural sciences appear to have a mystique, impenetrable to the uninitiated, often tends to generate an unconscious resentment in students of the scientific process who are not themselves scientists. This creates hazards for the management and support of science, both basic and applied, which increase as the total effort grows larger and more visible.

More broadly, we do not know how to measure the efficiency of science, either in relation to technology, or even relative to its own internal goals. As I pointed out in my paper in *Basic Research and National Goals* (5), we have few measures of the output of the research and development process or of its individual components. Considering the funds that the Federal Government devotes to such activities, a greater effort should be devoted to objective empirical studies of the process itself. Much of what is available is based on the personal experience of researchers, and is largely anecdotal. Social scientists are well aware that generalizations based on unevaluated, subjective experience can be very misleading, and even wrong. The Defense Department's Project Hindsight (6) is an example of an honest effort to study the output of research and development objectively. Unfortunately, some of the publicity that accompanied the publication of the summary report justifies the apprehensions of scientists regarding the premature and illegitimate deduction of policy implications from such studies. Although conceived originally as a study of management decisions in the research and development process, Project Hindsight was interpreted by some as a general indictment of the value of "undirected" research. Moreover, there was a failure to distinguish between "direction" from above and "motivation" by the broadly defined goals of the organization. In fact, the evidence from the study suggests that "motivated" research was considerably more successful in producing results than either "directed" or undirected" research. There are several other recent case history studies, such as those published by the Materials Advisory Board of the National Research Council (7). Several case histories are included in the present series of papers. It is important that the quality of effort in the "science of science" be raised. Too much current work depends on a misplaced emphasis on

statistical approaches such as counting of papers or of technological "events" without reference to quality or significance.

There may be value in having case studies that are conducted entirely internally within large research organizations as a form of self-evaluation or "technical audit." The lack of need to present a good "front" to a sponsoring agency or to higher management may encourage an intellectually more honest approach, and also permit the testing of more daring or tentative hypotheses or management innovations without becoming committed to them in a more public way.

It is important that some case histories, originally prepared by scientists or technologists themselves, be studied and evaluated by trained historians. The case for the utility of research is usually made on the basis of history, especially in the case of basic research. This is really the only solid ground we have, since basic research in general precedes its applications by ten years or more.

However, it is important to bear in mind that history may be an inadequate guide, since the boundaries between science and technology are becoming increasingly blurred. The decreased interval between scientific discovery and widespread application in recent years has been well documented. Furthermore, a number of social factors are progressively altering the nature of the whole technical enterprise—the growth in numbers of technically trained people as a fraction of the work force, particularly in management positions; the growth of higher education, especially the relative growth of graduate and postdoctoral training; the apparent increasing pace of adaptation of social and political institutions to technical change, at least on a small scale; the institutionalization of research and development as an economic activity; the appearance of a scientific equipment industry. In short, there appears to be a strong positive feedback inherent in the growth of science that increases the receptivity of society to the application of scientific findings and methods in almost every aspect of life (8). Historical studies have generally pointed up the fact that the development of technology has been surprisingly independent of the development of science, at least in detail. Yet most of the studies on which this conclusion is based come from the 19th and early 20th centuries, and there is evidence that the detailed interconnection of science and technology is becoming much closer, so that many of the most scholarly and solidly based historical studies may have the least relevance to the contemporary scene.

Industries such as communications, electronics, chemicals, pharmaceuticals, computers, and instruments are clearly science-based, and represent the fastest growing parts of our economy. Yet many scholarly studies of innovation are based on experience in the railroad industry, the metallurgical industries, the auto industry, or the farm equipment

industry. In many of these industries the underlying technology tends to be "observational" rather than "experimental," in the sense that it is difficult to do meaningful or relevant experiments except at nearly full scale with models of the final product. The paper by Goldman and Frey calls attention to the way in which this circumstance has retarded the application of science to manufacturing processes, owing to the high cost of meaningful experimentation.

The relative role of science and technology in the early history of the Industrial Revolution is well expressed in the following quotation from the German engineer Ferdinand Redtenbacher in about 1850: "The manifold mechanical movements needed for the arrangement of machinery need not always be invented anew. . . . A very exact and complete knowledge of mechanisms already invented is therefore most important in the arrangement of machines. Scientific knowledge is actually of little help, for complex mechanisms are evolved not through general powers of thought but by quite special powers of understanding of form, of disposition and of assembly of parts. Whoever is gifted with these powers and has developed them by varied practice will therefore be able to produce many and very ingenious inventions even though almost totally lacking in previous intellectual education; while he who lacks these powers, even though he have other most remarkable diverse gifts, will not yet be in a position to devise even the most insignificant mechanism." (From F. Redtenbacher, *Prizien der Mechanik und des Maschinenbaues*, Mannheim, 1852) (9).

The general approach to industrial innovation described in the foregoing paragraph is applicable to much of the period of rapid industrial growth in the United States in the 19th and early 20th centuries. It is not without importance today, but it is no longer the central style of innovation. The dominance of this outlook and style in the 19th century is illustrated by the fact that even Josiah Willard Gibbs, the greatest theoretical scientist produced by the United States prior to the 20th century, was awarded his Ph. D. from Yale in 1863 for a thesis on the design of gear trains, a thesis that relied heavily on geometrical visualization of the type described by Redtenbacher (10).

It seems clear today, however, that a new pattern is emerging in which the "general powers of thought" are replacing the "special powers of understanding of form" as primary generators of industrial innovation. This seems to happen less by a general uniform evolution than by the appearance and rapid growth of new industries with a new style of thought, beginning with the German chemical industry in the late 19th century and culminating in the modern computer, electronics, and communications industries. These industries were the first to develop a science base because their underlying technologies could be treated on a laboratory scale.

Studies by the *Scientific American* (11) show that there is a very high correlation between the rate of growth of an industry and its investment in science and technology. This does not necessarily mean that the research investment is the *cause* of growth; the reverse could well be true. But, as this difference in growth rate continues, and as new science-based industries nucleate and develop almost explosively, it seems clear that research-intensive industry will become an increasingly important segment of our economy. And further, these dynamic industries have a tendency to invade the older industries, as illustrated by the invasion of the textile industry by synthetic fibers produced by the chemical industry, or the invasion of electronics and computers into the machine-tool industry and, more recently, into publishing and educational supplies.

The point might also be made that, as technology becomes more sophisticated, it is created to an increasing degree by highly trained people who have a strong bias toward the abstract and the scientific. These people are increasingly penetrating all levels of management, and it seems likely that their viewpoint concerning the relation of science and technology will itself determine the future of this relationship, regardless of what the experience of an earlier era may have indicated about its nature. Each generation has its characteristic intellectual style, and in our own time abstract thought is quite clearly the dominant mode. Within the universities today, this is the mode that attracts the brightest students and the best minds, and there is evidence that the students are considerably in advance of the faculty in their adoption of this style. In the 19th century, Gibbs, the theoretical chemist, began his career as a mechanical inventor because that was the dominant style of his day, and even his most theoretical and abstract work shows the influence of this geometrical and mechanical style of thought. In the mid-20th century the intellectual style has been set by physics, especially theoretical physics, and there is evidence that this style is beginning to shift toward abstract mathematics. J. von Neumann, one of the great innovators of this century in both pure and applied science, shifted from chemical engineering to pure mathematics early in his career, and was internationally known as a pure mathematician before he turned his hand to technology. Norbert Wiener and Claude Shannon, also among the prime intellectual sources of modern engineering, were first-class pure mathematicians, although they had other inclinations as well. Computers already represent a technology dominated by mathematicians, and throughout many activities of industry and government we see technology increasingly concerned with "software" rather than "hardware," i.e., by information processing and the manipulation of symbols rather than by the processing of materials and energy.

### 3. Role of Government, Industry, and Universities in Applied Research

A number of correspondents in this investigation felt that there was a serious need in public policy for a better delineation of the relative roles of the Federal Government and industry in the support and performance of applied research. The Government was acknowledged to have a responsibility to support fundamental science, especially where it was connected closely with higher education. The Government also has a responsibility to support science that directly contributes to public purposes, such as defense, public health, weather forecasting, or environmental improvement. The responsibility of Government in the field of primary food production—i.e., agricultural and fisheries research—is also universally acknowledged. There was a feeling that the Government should not support research in areas in which private industry was active or could be induced to be active through suitable devices of public policy, such as tax incentives or the creation of new markets through purchase of products or services by public authorities. This feeling is based on more than a political bias in favor of free enterprise; it has a solid basis in the nature of the research and development process. Applied research is most effective when it is coupled to a "market" that provides an automatic measure of effectiveness of the end product of research. The existence of a market gives a continuous incentive for self-appraisal, which is often lacking for activities performed in the public sector. When the Government supports applied research in an environment that is not organizationally coupled to an end use, it is likely to stray from the mark, and this becomes more of a hazard the closer the research is to application. It is probably no accident that, by and large, Government-supported research has been most successful in defense, where the Government itself is the customer for the end product. An exception to this general statement is agriculture, where a slow evolution has resulted in extremely effective coupling between public research and private development, production, and marketing. Nevertheless, it is important to note that the Government role in agriculture extends well beyond the research itself to include extension services, marketing, economic services, and agricultural subsidies. The latter have had the effect of guaranteeing markets and thus to a considerable extent underwriting the economic risks of innovation. Another good illustration of a desirable pattern is provided by nuclear power. When research and development within the Atomic Energy Commission laboratories reached the stage at which successful development of civilian nuclear power plants seemed likely, the Atomic Energy Act was modified to encourage transfer of the new technology to industry as rapidly as possible, and the criterion of success became the willingness of public utilities to purchase

nuclear power plants following their own evaluation of the comparative economics of conventional and nuclear plants. Basically, the criterion for transfer was the willingness of private industry to take on the task, again really a market criterion.

Nuclear power was a development of such magnitude that Federal investment in the early stages of the technology was the only way it could be demonstrated. To put the matter in another way, the risks of the development were so high that only the nation as a whole could afford to bear them. The task of Government was really to reduce the technical risks to the point where they could realistically be borne by smaller social units. But direct subsidy of research and development is only one of several methods available for spreading risks. Others include tax incentives, allowance for the cost of independent research in the overhead on Federal procurement, guaranteeing to the seller a minimum size of market for an innovative product, Government underwriting of price stability (a very significant factor in agricultural innovation in the United States), guarantee by the Government of minimum performance standards to the purchaser (for example, in connection with new types of municipal waste disposal). The principle involved in each of these examples is that of maximum decentralization of the decision process, the customer having a strong incentive to make a sound and objective evaluation of the economics. A striking example of technical innovation stimulated almost exclusively by subsidizing a market rather than by direct Government support of technology is the case of uranium mining. With little more than a guaranteed price and market, the Government changed the known supply from extreme scarcity to abundance in about ten years. In fact, this alteration in uranium reserves—largely the result of private exploration, with some help from general geological knowledge developed by Government—was as important a factor in the commercial success of nuclear power as was Government-supported research and development. This kind of decentralization is also desirable on a smaller scale. For example, "spin-off" of small science-based firms from Government laboratories and larger industry occurs when technology reaches a stage at which the risks of further innovation can be borne by a smaller organizational unit than the parent organization. The process of spin-off is often assisted by the fact that the new entrepreneur may be partially supported by the parent research organization (11). A spin-off organization serving a specialized sector of the market is often more effective in the later or more applied stages of innovation than is a large organization with much greater technical resources. Only in this way can one explain the success of firms such as Itek, Polaroid, Xerox, or Texas Instruments.

The Federal Government should adopt a more hospitable attitude toward spin-off of new industry from federally supported technology, in-

cluding its own laboratories. There is still a widespread belief that ideas resulting from work done at taxpayer expense should be put in the public domain. However, this belief overlooks the fact that the innovator who develops an invention into a commercial product or process and tries it in the marketplace contributes as much or more to technological innovation and economic growth than the originator of the idea.

There remains the question of the role of universities in applied research. Historically the universities have been the major centers of applied research in both agriculture and medicine, although in both these cases a large corollary development activity has grown up in industry. The university research activity has been well coupled to the operational use of the results. In the case of agriculture, this has occurred through the experiment stations and through the extension service, which have made it possible to demonstrate the economic value of the research results rather directly. In medicine the demonstration activity has occurred through the affiliated teaching hospitals. Thus one may generalize by saying that a fairly effective system of technology transfer has grown up in the life sciences, which has made it possible to couple applied research in universities to the ultimate user. Although some universities have developed engineering experiment stations, there is not for the most part a strong tradition of applied research in the physical sciences corresponding to that in the life sciences. This results largely from intrinsic differences between the applied life sciences and engineering. Since living systems always exist in many nearly identical exemplifications, a discovery or invention in the life sciences, even when highly specific and applied, also has a high degree of generalizability. A new technique for a surgical operation can be applied immediately in many nearly identical circumstances. A new variety of seed or a new method of cultivation can be disseminated rather readily, and there is often not a large problem of scale-up from the laboratory to operational use. Where there is such scale-up, as in the case of fertilizers, pesticides, drugs, medical instrumentation, or farm machinery, the corresponding development work has been most effectively done by industry. Thus we see that applied research in the academic environment is most effectively done when it is readily generalizable and where problems of scale-up or large-scale production are not of major importance. Such problems usually involve careful timing, scheduling, or programming of research, which tend to be incompatible with the other requirements of the academic environment.

The problem of scale-up involves more than physical size, however. Of equal importance is the problem of scale-up in complexity or "intellectual size." The development of complex systems involves the coordination of many component pieces of a problem and many individual specialities. Often it involves highly sophisticated science or mathematics side by side with rather conventional or mundane design or repetitive

analysis. Such a coordinated effort tends to be incompatible with the university environment, with its high turnover of people, its treatment of research as a part-time activity, and with the high value it places on individual as opposed to team performance, and on the proposing of new ideas as compared with critical evaluation and comparison of ideas and their execution in all the most mundane detail. In the future we may expect more enterprises in the life sciences to partake of the same complexity that is now characteristic of many engineering systems. Thus the increasing significance of "intellectual size" in these areas may generate greater reliance on mission-oriented institutions only loosely associated with universities, or completely separate.

When engineering close to production has been done in universities, it is usually in separately organized and staffed contract research centers having a quasi-industrial character. The close association of such centers with universities or technical institutes does assist in recruitment and also provides a source of valuable applied experience for faculty and graduate students, though often to a relatively small fraction and on a somewhat haphazard basis. The operation of both contract research centers and engineering experiment stations or institutes has been attacked as competing unfairly with private enterprise, and recently there has been a strong trend of opinion both inside and outside universities against the operation of contract centers for applied research by educational institutions. The responsibility for staffing and administering such centers throws a load on already overburdened university administrations and diverts them from tasks more central to their educational and basic research missions. It often involves the university in direct competition with industry for contracts, and in making evaluative judgments on sub-contract performance by industry. If the research is under security classification, or involves dealing with proprietary information, it departs from the academic tradition that all scientific activities that are proper to a university should be open to the free and searching criticism of the entire world scientific community. There is an often-justified fear among university faculties that security classification will be used to cover mediocre, routine, or pedestrian work.

When applied research in universities has led to useful new technologies, it has often been that the research was undertaken to serve a purpose internal to the university, or where the application was a direct extension of basic research. Early computer development was carried out in several universities largely for the purpose of providing a better tool for scientific computation in basic research. The nuclear resonance spectrometer, the atomic clock, the maser, and the laser were all logical extensions of basic research already under way. The high-power klystron was developed for accelerators for nuclear research. Some fundamental technological developments, particularly in materials and in chemistry,



have come from applied university work. Here again, this has usually happened in areas in which the problems of scale-up from the laboratory were minimal. The universities continue to be major sources of innovation in computers, especially on the software side, though the center of gravity has probably shifted to industry.

In general, I believe that more applied research in universities is desirable, when it is appropriate. One might state a general principle as follows: When basic research is to be supported, the burden of proof should lie with those who wish to place it outside the university; when applied research is to be supported, the burden of proof should lie with those who wish to place it inside the university. The following criteria favor university performance of applied research:

1. The results are readily generalizable, as in medical research.
2. The research lends itself to involvement of students, i.e., it is not programmed or scheduled to meet deadlines.
3. It is unclassified and not subject to publication restrictions, and thus open to full scrutiny by scientific peers everywhere.
4. It is a logical extension or outgrowth of basic research under way or already performed.
5. It is of primary benefit to the public sector, or relates to areas of public responsibility.
6. The inventor is on a university faculty.
7. It relates to the development of a fundamentally new technological capability, involving new principles, and of benefit to more than one company or industry.

It is usually desirable that applied work begun in universities should be transferred to industry, where appropriate, as soon as possible, and certainly prior to manufacturing or operations.

On the other hand, in considering what type of institution is appropriate for what type of applied research, an overriding consideration may be the source of the original idea. Experience indicates that an idea seldom thrives if taken out of the hands of its inventor at too early a stage, and invention does not always follow organization charts or formal definitions of mission.

#### 4. Distinction Between Science and Technology

In this area there is a wide variety of views ranging from the opinion that science and technology are really quite separate to the opinion that technology is essentially applied science. The truth is probably intermediate. Part of the variety of views may be due to variations with time. As I have suggested in Section 2, much of the technology in the 19th century owed little to contemporary science. On the other hand, an increasing component of today's technology is closely dependent on

science—if not on contemporary science, then on science 10, 20, or 30 years old—but theoretical science nevertheless.

Even for the 19th century it is easy to exaggerate the independence of science and technology. Although this tended to be true of mechanical invention, it was less true of the applications of electricity and chemistry, even then. For example, a working model of an electric generator was constructed only a year after Faraday's discovery of electromagnetic induction, and an electric motor two years later (12). That Faraday's discovery did not immediately turn into a major industry was not due to failure to realize its technological potential, but rather to the fact that a whole complex of other inventions for the utilization of electricity would be required before an economically viable technology could be created.

It is true that there is inevitably a considerable component of "art" in technology. Technology is essentially a codified way of doing things, and much of this is based on systematic theoretical knowledge, which is science, but some simply on codified experience, which is what I mean by "art." A good technologist must sometimes be willing to accept or search for solutions that work, even if they are not fully understood. In this he is not so far from the experimental pure scientist who often behaves like a technologist with respect to his own experimental techniques. In fact, each branch of science is based on a characteristic technology, which changes as the science advances. On the other hand, the greatest impact of the scientist in an industrial environment has resulted from his unwillingness to accept rules of thumb or procedures that are not understood.

The technology associated with an experimental science tends to be passed from worker to worker somewhat independently of the conceptual scheme of the science. There is a collection of "tricks of the trade," which lie outside the body of formal scientific literature. Technologies developed for scientific purposes often later grow into technologies useful for industrial or other operational purposes. Research instruments are first commercialized, then used in other sciences, and finally used to control production processes. Laboratory tools and techniques such as high pressures, cryogenics, high vacuum, spectroscopy, vapor-phase chromatography, and so on, begin in a research laboratory but often end up on the production line. One of the most dramatic examples is the cathode-ray tube which, originating as a physics laboratory device, became the basis of the modern television picture tube. These experimental technologies undergo transformation and improvement in the process of being applied, but their origin in experimental pure science is still evident.

It also happens, of course, that technologies developed for applied purposes are later turned to providing new instrumentation for pure science. World War II produced a host of new techniques, especially in connection with microwaves, that have become indispensable laboratory

tools of physics research, and more recently of physical chemistry. Within the last ten years some of the tools of pure science have become major engineering projects in their own right. The most dramatic examples are high-energy accelerators, satellites for instrumented space exploration, modern radio-telescopes, and the Mohole project (until its tragic demise). In addition, Government-supported pure research has created a large commercial market for research instrumentation, including moderate-size accelerators for low-energy nuclear physics.

There are cases in which it may be desirable to develop a field of pure science partly for the sake of the by-product technology that it generates. Although, in principle, this technology might be developed for its own sake without the associated science, in practice the scientific end use provides the focus and motivation, which generalized development could not do. In addition, it attracts more dedicated and able people through the intellectual challenge of the science. Often the other uses of the tools so developed do not become apparent until after the development has been completed. A dramatic example is the oceanographic research submarine *Alvin*. Designed with the needs of basic oceanography in mind, it was used with its research crew to locate the lost hydrogen weapon off the coast of Spain in 1965. Today, in elementary-particle physics, the requirements for information-handling of the tremendous volume of data obtained are stretching computer technology in a way that is advancing the whole art. Already techniques of "pattern recognition" originally developed for automatic scanning of cloud-chamber photographs of nuclear-particle tracks are finding application in other areas, such as automatic letter sorting. Nuclear physics and, more recently, radio astronomy, are remarkable for the complexity and volume of the systems of data with which they have to cope. On the other hand, it would be dangerous and misleading to argue for support of these fields solely or even principally because of their demands on data processing. Nevertheless, these by-product effects should properly be considered in assessing priorities or in assessing the relevance of a field to applications. Another example may lie in the field of sensitive optical or infrared detection for the purposes of astronomy. While one would not undertake such technological developments unless they served a very important scientific purpose, their other uses may be of great significance, and the cumulative effects of such technological developments emerging as a by-product of support of pure science over a long period may be very large.

Numerous observers have commented upon the differences between the communications systems within science and those within technology. Science has an elaborate system of public documentation with strong sanctions operating on the individual scientist to make full use of and give proper credit for previous work relevant to his own. Discovery and

innovation within science have extremely rapid diffusion times, and the rate of diffusion is influenced to only a minor degree by political and organizational boundaries. There are somewhat higher boundaries between scientific disciplines, but even this is often exaggerated by outside observers (13). Within technology the communications pattern tends to be more localized and more confined to organizational channels. One reason for this, of course, is that much technological innovation is harder to verbalize and to document. The intuitive aspect of invention, so eloquently described in my earlier quotation from Redtenbacher, makes it more dependent on face-to-face contact and learning by doing. Thus, typically, in an industrial or Government laboratory the scientists tend to be oriented toward external communication and toward recognition and appreciation by the outside professional community, while the technologists tend to be oriented toward internal communications and toward recognition and reward within the organization. I believe a changing pattern is discernible in this respect, however. The newer technologies such as nuclear energy, electronics, and computers tend to be more externally oriented than the older technologies. There is more conscious effort to conceptualize technological knowledge. Although invention in the sense of intuitive synthesis may be just as important in the newer technologies as in the mechanical technology of the past, there is a greater tendency to document and to generalize specific advances—for example, to convert the conception of a particular device into a design theory or into a set of formalized design principles for a class of devices or processes. In my opinion, this is an important trend, and one that is highly desirable from the standpoint of accelerating the application of science and the diffusion and adoption of technical innovations.

In the technology of medical care, the tradition of documentation of practical advances is somewhat better established than in engineering and industrial technology. This is partly because, as mentioned earlier, medicine is dealing with the same human body everywhere, so that medical procedures may be more generalizable than engineering procedures, which relate to the design of particular products or processes.

A particular problem in the interaction between science and technology has been eloquently described by Peter Drucker (14). It is the reluctance of technologists to deal with the more mundane and less sophisticated problems, which still may be quite important socially. This is a special difficulty in connection with the transfer or adaptation of technology to underdeveloped countries, but it is also an inhibition against application of technology to the more backward civilian industries in our own country. The problem is probably closely related to the discussion by Cyril Smith, in his paper in this series, of the reluctance of scientists to deal with complex or "messy" systems. The inhibitions are undoubtedly associated with the fact that the solutions, even when

successful, are seldom elegant or intellectually satisfying. The importance of such problems is not in itself sufficient motivation for attention when there are comparably important problems in more sophisticated areas that give greater intellectual satisfaction.

### 5. Applied Research and Basic Issues

As applied research and development are more and more performed by people with original training in basic science, and thus interested in and aware of basic issues, applied research is likely to bring increasing benefits to science itself, as well as to technology. Applied research will continue to turn up important basic issues that the discoverers will increasingly be capable of recognizing and pursuing. This will be recognized also as having benefits for technology itself, for when applied problems are approached with the methods and the generalizing tendencies of basic research, the solutions found tend to be more broadly applicable, or to lead, by "serendipity," to new applications. Applied research must often look beyond the time horizon of the immediate purpose for which it is undertaken. The more sophisticated the field of application the less likely it is that the first version of a new invention will be valuable without much further development. It is this further development that applied research aimed at deeper understanding of the underlying phenomena is especially important. For example, the first discovery of the gas laser at Bell Laboratories was followed by an intensive period of rather fundamental research in atomic and molecular physics, which eventually led to greatly improved lasers, culminating in the development of the CO<sub>2</sub> laser with a power output several orders of magnitude greater than that of the earliest gas lasers.

A fundamental problem in the education of the modern applied scientist is how to train him to bring a basic research viewpoint and approach to applied science without creating in him a disdain for, or impatience with, applied problems. A frequent shortcoming of the basic research viewpoint is a tendency to view all problems in the light of the researcher's own specialty. Although basic research training is undoubtedly the best possible type of training for the ablest people, who are able to turn their attention to many different classes of problems, applying the intellectual techniques they have learned through deep study of one specialty, the less capable people may tend to become prisoners of their specialized training, choosing specialization as the best way to exploit their more limited capabilities. In the best of American graduate education this is offset by requirements of a certain degree of breadth in course work, but it is a constant hazard of graduate work—paradoxically enough, even more often in engineering than in the sciences.

Enlightened industrial laboratories often adopt the practice of encouraging newly hired Ph. D.'s to tackle problems quite remote from the

areas of their thesis research. The value of graduate training should lie partly in the confidence it instills in the student to solve new and challenging problems, and to assemble independently the information and tools necessary to do it; yet too many students want to use their first work assignment as an opportunity to extend and improve upon their Ph. D. theses, rather than to broaden their experience and skills.

## 6. Involvement of Non-Scientists in Technological Judgments

When the experts disagree on the correct technical course to take, the decision between alternatives is often thrown back on the legislator or non-scientific executive. The question of the proper degree of involvement of the non-expert in technical judgment is one of continuing controversy. As with all arts, executives and legislators with long experience develop a surprising talent for ferreting out key technical issues, without understanding the technicalities. The congressional hearings on the Nuclear Test Ban Treaty provide a classic example of how it is possible for the experienced generalist to elucidate the key issues without necessarily understanding all the underlying science. Similarly, examiners in the Bureau of the Budget, though trained as political scientists or economists, acquire an uncanny "nose" for the important issues without really being able to argue the technical merits.

Furthermore, many of the types of questions that legislators or executives are required to answer are really questions of political preference, which are only slightly disguised as technical issues. Most commonly, important decisions in applied science depend not on technical feasibility, which is uniquely the province of scientists and technologists, but on social desirability, which must be determined by a multidimensional interaction of scientists, technologists, public servants, and the public. In practice, questions of technical feasibility and cost interact with desirability, and hence the need for a many-sided discussion. A good example of the type of complexity involved is the case of the supersonic transport. Real dangers are involved, however, when the non-scientist attempts to impose his own value system on what should be largely scientific decisions. The public is often tempted to dump large amounts of money into the solution of problems that are perceived to be of social importance, without adequate consideration of feasibility or economic efficiency, and without adequate understanding of the interrelationships within science. The result is sometimes an assignment of relative priorities, which actually diverts resources from what might be the most promising lines of advance. The national investment in aircraft nuclear propulsion is probably one of the most striking examples of such misapplied effort. There is a special hazard of misconceived priorities in the field of health, in which the most "visible" diseases tend to receive the greatest

research attention. In times of rapidly increasing budgets this is not a major problem, because there is usually enough surplus to cover the less spectacular but scientifically more promising efforts. Furthermore, individual scientists have their own sense of priorities, and the most creative people tend to allocate their effort to the most productive fields as long as some support is available. The danger comes when funds become so tight that narrowly conceived applied efforts completely pre-empt the field.

In the past 15 years the legislator and non-scientific administrator have tended to adopt a "hands off" attitude toward scientific decisions, probably more than is desirable even in the long-range interest of science itself, but currently the pendulum seems to be swinging rather violently in the opposite direction. The non-scientist often tends to see utility in rather narrow terms, and can be impatient with what appear to him to be diversions from the principal social goals in the name of science. There is a sort of corruption here which is dangerous for science and technology. The availability of large resources for efforts of apparent social importance may tempt scientists to make expedient promises of quick utility in order to obtain support for work they wish to undertake. It is always relatively easy to invent new terminology to label fundamental scientific work with perfectly legitimate "applied-sounding" words. But the unfortunate aspect of this is that, usually, the less the ability and integrity of the scientist the more willing he is to invent expedient labels for his work, so that the net effect of providing support preferentially for fields or projects that have the appearance of immediate social utility is to drive the best and most creative minds out of the field. In many instances it may be better to support fully a first-class scientist who is willing to devote part of his effort and thought to the applied needs of an agency than to support several mediocre scientists who are willing to devote their full effort to the problem as defined by the agency. Intellectual freedom is more valuable than money in attracting first-class people into socially important fields, and the Congress would do well to keep this in mind when providing funds for applied work. The more narrowly the objectives of a program are defined, the more likely it is to drive out the most creative workers. Even for applied work, the system of research evaluation and project selection by judgment of peers is a powerful antidote to the waste of money on work that is spuriously claimed to have immediate social utility.

On the other hand, I would agree with those who say that scientists and engineers have a much greater obligation than they have assumed in the past to explain their work in terms that are intelligible to the non-expert and the general public, without being condescending. Too many scientists confuse simplification with condescension. There is good intellectual discipline in explaining oneself to people not committed to

one's own specialty. However, it is essential that short-term support decisions should not depend primarily on *annual* justification to non-scientists. The cycle time for such justifications should usually be several years.

The postwar era was characterized by heavy attention to technical problems related to national security—defense, atomic energy, and space. These problems are technology-limited to such an extent that social and economic considerations are of minor or negligible importance. The characteristic institution of this period was the agency defined by an area of technology, e.g., the Atomic Energy Commission or the National Aeronautics and Space Administration. In the last few years, public and political attention have turned toward problems having both technological and social components, usually in complex admixtures: transportation, urban reconstruction, pollution, education, and industrial growth in lagging sectors. Even in the defense area, the shift of emphasis to limited war has brought in a much larger social component; the field of health, the only other area of major Federal research investment, partakes of some of the characteristics of defense, being largely science-limited at the present stage. In health, however, emphasis is shifting from the understanding and cure of disease somewhat toward the organization and delivery of care, again having a larger social component. For such society-limited problems, a factor of considerable importance is the social acceptability of solutions to many people, something which was of little or no concern in the Apollo program or the Minute-Man weapons system.

The distinction between science-limited and society-limited problems is not invariant with time, and may in fact be radically altered by technological progress, as Weinberg emphasizes in his paper. A new technology can overcome social limitations in several ways: by drastically reducing the cost of certain operations or products, by greatly simplifying certain products or operations and thus making them more accessible to the average individual, or by developing a wholly different way of doing things that does not have the same side-effects as existing procedures. Classic examples of cost reduction were the invention of barbed wire, which made fencing economically feasible in the American west; mass production, which made personalized transportation available to the average man; and automatic telephone switching, which made electrical communication accessible to practically everyone. An example of simplification is the IUD (intra-uterine device) which has greatly reduced the motivation necessary for family planning in underdeveloped countries. Examples of alternate ways of doing things are the introduction of nuclear power, which makes possible energy generation without atmospheric pollution, or the development of biological methods of pest control, such as male sterilization, or, in the future, species-specific hormones,



which permit elimination of pests with less serious environmental consequences than do general-purpose chemical pesticides.

Inventions that permit "designing around" social obstacles require just as much social ingenuity as technical ingenuity, and often the two have to be combined in a single individual. The process of inventing a product for a market is usually one that requires both technical and social invention. The perception of a market possibility consists in seeing what kind of technological invention is needed to overcome a particular social barrier. There are times when stating the need for a particular invention without any knowledge of how it can be done technologically may be a much more important step than the technological solution itself. This was very clearly understood in the 19th century, when many inventions were of this character and required relatively little sophistication in technology.

It is perhaps a hazard in today's highly sophisticated world that preoccupation with technology—a preoccupation made necessary by the high level of education required—may result in too little recognition of the equally important necessity of properly articulating social needs, or, if you prefer, the requirements of the market. With respect to the great modern problems—what I call the four P's of population, pollution, peace, and poverty—it may be that articulating these is the most important part of the problem: that once these needs are formulated in the right way, the technological solutions will become obvious, or will fall into place. However, it is important to note in this connection that what is required is the articulation of social needs in such a way that one can get from where we are to where we want to be without the necessity for massive persuasion or massive education of many people simultaneously. Technology can lower the "activation barrier" for such a process so that we can get from where we are to where we want to be by running downhill gradually rather than by crossing a mountain pass of social acceptance.

## 7. The Mission-Oriented Laboratory

The characteristic institution for the conduct of applied research in the modern era is the large, multidisciplinary "mission-oriented" research organization. Although this type of organization has not replaced the small specialty company or even the independent inventor as a source of innovation, it is to an increasing degree the source of basic technology both for public purposes and for industrial projects. The term "mission-oriented laboratory" comprises several different types of institutions:

- a. Large Federal civil service laboratories, such as the major laboratories of the Department of Defense, the various National Aeronautics and Space Administration centers, and the pioneering laboratories of the Department of Agriculture.

b. Federal contract research centers operated by universities, university research associations, or separately organized non-profit corporations. Examples include the Lincoln Laboratory of the Massachusetts Institute of Technology, the Los Alamos and Livermore Laboratories of the University of California, the RAND Corporation, the Aerospace Corporation, and the Institute for Defense Analyses.

c. Central industrial laboratories or Federal contract industrial laboratories such as the Bell Telephone Laboratories, the General Electric Research and Development Center, the Sandia Corporation, or the Oak Ridge National Laboratory.

d. Multi-purpose research institutes or research service industries such as the Stanford Research Institute, the Battelle Memorial Institute, or the A. D. Little Company. These organizations provide research services to clients on a job-shop basis.

I have deliberately not included in this classification research institutes whose principal "mission" is the advancement of a field of pure science rather than the development of technology or the fulfillment of some social mission such as defense, health, or food supply. Examples are the Brookhaven National Laboratory, the Carnegie Institution of Washington, the Smithsonian Institution, the Woods Hole Oceanographic Institution, or the National Radio Astronomy Observatory. Admittedly, the line between these and mission-oriented laboratories is sometimes hard to draw, since the mission-oriented laboratories carry out much fundamental research bearing on their mission, while the scientific institutes often move close to technology or utilize technology in the design and development of their research equipment. An organization such as the Jet Propulsion Laboratory of the California Institute of Technology is especially difficult to characterize, since its operations are largely technological, but its basic mission essentially scientific.

What constitutes a "mission"? How is it defined, and how is it used to shape the specific research program? How is success in the performance of a mission to be measured?

The answers to these questions are complex and often subtle. A mission must be neither too vague nor too specific. It must be concrete enough to provide real guidance in the choice of tasks and priorities, and to be understandable by the key people in the organization, but it must be general enough to permit the phasing-out of old tasks and the establishment of new research goals. A mission must be like the shell of a building, within which the interior can be drastically rearranged to carry out constantly changing tasks. A mission, however, should not be simply an umbrella under which almost any high-quality scientific activity can be justified. Not every exciting discovery is convertible into an economically or socially useful product. Unfortunately, the broader the objectives of an institution, the harder it is to determine what is really relevant to

its mission. Very large diversified companies find that almost everything is relevant in principle, but they have to pick and choose, at least in the short run, in order to achieve "critical size" in the efforts they do support. In many cases it may be more important to maintain this critical size than to "cover every bet." One reason for this is that the transfer of information between organizations occurs more rapidly, except under conditions of secrecy, than does the vertical transfer from research or invention to marketable product. In the research part of an institution, it is sometimes more important that the organization be working in a general field than that it be working on a particular project. A company—or for that matter a nation—that has a broad technical capability can quickly exploit the ideas of others, and can catch up on the bets that it misses provided it has the technical sophistication to identify promising ideas at a sufficiently early stage. The example of the tunnel diode, discussed in the paper by Suits and Bueche, is a good illustration of how an organization can rapidly convert an external discovery into a valuable product. It happened essentially because there were people inside General Electric who were capable of making the same discovery—indeed might have made it. From the standpoint of the effectiveness of the organization, it was more important that they were capable of making it than that they actually made it, because this guaranteed that they would recognize the significance of the discovery as soon as it appeared. Just as a company or a nation cannot expect to exploit every promising scientific discovery, so every discovery that it exploits need not be its own.

In considering the "missions" of Government laboratories, it is essential to distinguish a "mission" from a "task." A mission is a function assigned to an organization by higher authority or by legislation. A task is a subordinate objective that is best generated from within the research organization and pursued, usually by agreement with the sponsoring agency. A research institute that does not generate most of its own tasks, but depends on external direction or "orders from headquarters," is either suffering from inadequate leadership or has a mission which is inadequately defined.

The definition of its mission is one of the most important considerations in establishing a new research organization or reorienting an old one. In evaluating the performance of such an organization in applied research, the emphasis should be on the performance of the organization as a whole rather than on its individual components. Good applied research is of little value if the mechanisms do not exist to translate research results into goods, services, or operations. A frequent paradox observed in civil service laboratories is the high level of scientific performance of individuals contrasted with the often disappointing results from the organization. A good scientific performance often does not add up to an effective overall performance, partly because of the cumbersomeness of the decision-

making process, and partly because of poor communication between the working scientist and the headquarters organization that supports and administers his work. Too often the insights and understanding of the working scientist are distorted and transformed by many levels of intermediaries, each perfectly competent and honest in itself. Scientific advice transmitted through many levels of management loses its intellectual integrity, and hence its effectiveness. For similar reasons, the working scientists in Government laboratories fails to acquire a comprehensive view of the goals of his laboratory and how his efforts contribute to them. Not fully appreciating or understanding the broad goals, he is unable to identify the key tasks and to establish his own set of priorities consistent with the broader goals. These faults are by no means confined to Government laboratories, but the strongly hierarchical nature of Government tends to aggravate the problem. Too often in Government, initiative and reporting in a scientific organization are associated with position rather than with specific subject-matter competence.

There are certain identifiable characteristics of successful mission-oriented laboratories that seem to be independent of whether they are located in Government, industry, or universities. These characteristics are more related to the "sociology" or the communications pattern of the institution than to its formal organization. I have listed some of these characteristics below, with a brief discussion of each:

1. Full awareness and general acceptance of the principal goals of the organization by its key people.

Even though the motivations of the sponsor and the scientist may differ, the scientist is aware of the main problems and goals of the institution. The goals are arrived at by discussion and argument and are usually more the result of consensus than of directives passed down from the head, although the consensus may be formally ratified by such directives. Successful organizations have a tangible "organizational memory," a sort of collective recollection of enduring themes and problems.

2. Willingness to consider and implement new ideas and initiatives on their own merits, regardless of the organizational level at which they originate, or whether they come from inside or outside the organization.

The phenomenon of "not invented here" is one of the worst enemies of innovation, and one of the hardest to overcome. The many case histories discussed in this series of papers show that no part of an organization has a monopoly on new ideas. Furthermore, ideas must be nurtured in their early stages by the group that originates them, regardless of its formal task or mission. The originating group should be permitted to carry a new idea to the point where it is either proved unpromising or sufficiently demonstrated to convince a more appropriate part of the organization to adopt it.

3. Mobility of people between the more fundamental and applied activities of the organization.

As emphasized frequently by our correspondents, as well as by the papers in this series, people are the best carriers of technology transfer, and therefore the organization should be flexible enough so that people can move with ideas and technologies. Many mission-oriented organizations take on young scientists as basic researchers and then simply permit the environment to attract them into more applied activities as they gain in maturity and experience. Since new technologies frequently derive from laboratory techniques, it is natural that the young basic scientist of today may become the successful applied scientist of ten years from now, provided the environment is such that he feels rewarded by being able to contribute to the applied problems of the organization. Not all basic researchers will move in this way, but a considerable percentage will, and will be replaced by new young scientists.

4. Quick recognition and funding of new ideas, at least to the point of ascertaining the desirability of a larger commitment.

This is probably the single most important factor in technology transfer. Studies of innovation indicate over and over again that the successful ideas are nearly always those for which initial funding was obtained promptly and with a minimum of review by higher authority. Obviously major commitments have to be reviewed, since no organization can develop more than a small fraction of the promising ideas it originates, but each level in the organization should have sufficient flexibility in funding to be able to commit some fraction of its budget to a new idea on its own responsibility.

5. Extensive freedom at each organizational level in the organization to reallocate the resources within the relevant area of responsibility.

This is, of course, a close corollary to number (4). Each level of management in research should have the largest possible measure of control over all the resources needed for effective performance of its job. Multiple functional restraints in travel, telephone, supplies, personnel ceilings, etc., which are not directly relevant to assigned task responsibilities, should be avoided. In Government laboratories such restraints are often imposed by administrators or by legislation as a form of cost discipline that substitutes for the more automatic cost controls that operate in the private sector, subject to the discipline of the market. In practice, however, such controls frequently lead to inefficiencies in research performance, whose cost far exceeds the savings resulting from the controls. The situation in Government laboratories has improved greatly in recent years, but much remains to be done to give the laboratory or program manager greater freedom to use his resources under an overall fiscal ceiling. This is not to say a manager should not operate under a

functional budget, but this budget should be a guide and not a strait-jacket, and each reallocation among functional categories should not be subject to prior review by higher authority. The proper substitute for functional constraints is better definition of substantive goals, and measurement of performance against these goals at each level. Furthermore, if people in a laboratory are to be realistically measured against such goals in their performance, they themselves must have a part in setting the goals, as I have indicated under number (1) above.

6. Full communication through all stages of the research and development process from early research to ultimate user.

The various stages of the research and development process should overlap substantially. The studies under Project Hindsight showed that many important innovations in weapons systems occurred after the design was fairly well committed. This underlines the importance of continuing research well after designs have been frozen, as a hedge against unforeseen problems or difficulties. Similarly, wherever possible, the ultimate user should be brought into the picture as early as possible. This procedure has been especially well developed in the chemical industry, where the operator of the plant often joins the design team at an early stage. The research and development people should be encouraged to follow products and processes into ultimate use in order to obtain "feedback" on problems and potential improvements.

7. A good organizational memory for the enduring technological problems and themes related to the broad mission of the organization or laboratory.

It is this organizational memory that most distinguishes a mission-oriented laboratory from a university or basic research laboratory. In basic research, memory and continuity tend to be deposited in the scientific literature and the professional communications system rather than in a particular organization. Suits and Bueche in their paper emphasize the role of this organizational memory in several of their case histories, especially that of the vacuum switch. It is partly what distinguishes the "professionalism" referred to by Bode in his paper. Of course, this memory can also become a dead hand, a knowledge of what can't be done, if it is not combined with a high receptivity to new ideas. Mission-oriented laboratories must thus combine continuity and mobility of personnel. Some people will move with new technologies; others may live out their careers with particular constellations of problems or themes.

Isolated discoveries are of little value unless they fit into a communications pattern. This can be either the pattern of the scientific community or that of the "mission" of the institution. Science is transformed into technology through the intersection of the scientific pattern of communications with the institutional or mission pattern. It is the institutional environment that provides the mechanism for identifying the significant

problems—significant, that is, in terms of the mission—and this is where the importance of the organizational memory lies.

8. A system of recognition and reward that assigns highest significance to technical contributions to the goals of the organization.

This is also a corollary of point number (2) above. If the sources of innovation are not associated with position in the organization, then it follows that the system of reward and recognition should not be associated exclusively with organizational responsibility. The individual who makes an outstanding technical contribution, but prefers to continue in the laboratory with a few colleagues, should be as eligible for recognition, financial and otherwise, as the individual who manages a large group. A more subtle question is that of the degree to which institutional recognition should reflect external professional recognition. Certainly, professional reputation must be an important factor, but the mission-oriented laboratory should offer comparable rewards to the individual who chooses to follow the pattern of the internal or institutional communications system rather than the professional one.

A continuing problem for large laboratories that are not directly coupled to an industry or an operating organization is the age structure of the laboratory. This problem becomes especially acute in the case of government laboratories, whether under contract or in the civil service, because of the relative lack of mobility of personnel. Many such laboratories have grown rapidly in recent years and have a relatively young average age, but now there is a tendency for growth to level off, and with it the intake of younger scientists. As the average age of the laboratory increases, its outlook and style tend to change. It becomes more "professional" in the sense of having a deeper background in its assigned technology and mission, but it also tends to become more conservative and resistant to new ideas. The lack of infusion of young scientists with new techniques and viewpoints acquired from their training lowers the innovative spirit of the organization. When the laboratory is coupled to an industry, a certain proportion of the people move on to other company activities, either more specialized manufacturing support laboratories or operating organizations. Here they infuse a new spirit into company management and operations while leaving room for new blood to come in at the bottom in the research activity. Similarly, in universities there is a constant flow of young people through the lower ranks of the organization, and, although the system of tenure introduces some of the rigidities characteristic of the civil service personnel structure, there is generally greater mobility in universities than in mission-oriented laboratories.

However, aging of the staff is only one of the pitfalls of such laboratories. Their mission may become obsolescent, due either to technological progress in other areas or to changing external requirements and em-

phases. Yet these changes can be very subtle. A laboratory can retain much of its original technical competence, while its mission is no longer quite adequate to fully engage its capabilities. Under such circumstances, the most creative people in the organization may tend to leave or to drift more and more toward basic research as a substitute for other mission goals. While basic research is essential to the technical vitality of a mission-oriented laboratory, it should not become an end in itself in that setting.

### 8. Status of Applied Science in the United States

After many decades of inadequate attention to basic science, and inadequate recognition for scientists, the United States may have over-reacted. Raising the status of applied science in universities has become a real problem, as both our correspondents and our authors have suggested frequently. There has always been a kind of status hierarchy of the sciences, in order of decreasing abstractness and increasing immediacy of applicability. This hierarchy begins with pure mathematics and runs to theoretical physics, experimental physics, chemistry, biology, engineering, medicine, social sciences, and so on. Even within a single discipline, one finds a kind of hierarchy between the pure and more applied practitioners of the discipline. In a sense, what has really happened is a disturbance of a natural equilibrium that has existed for a long time. Historically a certain snobbery has always existed between pure and applied science, but whereas once the exponents of pure science represented a small and rather ascetic minority who took pride in sacrificing the greater material rewards of applied science for what were thought of as the greater psychological satisfactions of pure science, today the gaps in both material reward and external prestige between basic and applied science have largely disappeared. However, some of the mythology of "we happy few" in basic science has persisted. As pointed out by many observers, the values and attitudes of the academic "subculture" are increasingly becoming the dominant values of the influential segment of society as a whole, perhaps inevitably in a time when more than 50 percent of high school graduates go on to college. The business tycoon of the last generation boasts of his son's achievements as a theoretical physicist or a Sanskrit scholar, and economic motivations no longer play as large a part in affluent middle-class society.

Historically, in the United States, training and recruitment of people for the applied sciences has usually been associated with the progressive democratization of higher education. The most dramatic example is the establishment of the land-grant colleges and universities. But in more recent times it is notable that the G.I. Bill resulted in the recruitment of thousands of able students into engineering and other applied sciences.



With the exception of medicine, entry into the more applied aspects of science has always been an important path for upward social mobility in American society, and the applied sciences and engineering have flourished in periods during which political and social forces have brought new groups of people into the professional and technical labor force. Applied science benefited greatly from the influx of people that occurred as a result of the wartime mixing up of the normal status relationships of American society. It may well be that the present drive toward equality of opportunity in education will prove one of the most important and beneficial influences in raising the level of applied science in the United States in the coming years.

One cannot discuss the question of basic versus applied science without recognizing that there is some objective basis for the snobbery that exists among academic people. This does not mean that it is right or justified, but, on the basis of numbers of people alone, it is inevitable that on the average basic research will have a higher percentage of able people than applied research. Since there are fewer basic researchers, the group is more highly selected. Furthermore, basic research on the average probably is more demanding than applied research. This is simply because much applied research can be directed and organized to a greater degree than basic research; thus people with high skill in the *techniques* of a discipline, but little creative imagination, can perform very well in applied research, given imaginative and energetic leadership. The catch is that the leadership function in applied research is probably *more* intellectually demanding than that in most basic research. Thus the professor who advises his less-able Ph. D. students to go into applied research may be acting from a sound instinct. Under first-class leadership, his less-able student may have the more productive career in applied research, especially in comparison with academic research, which tends to be highly individualistic. The professor's advice, while sound from the standpoint of the individual student, may adversely influence the general climate of opinion among students, and ultimately, colleagues.

While I agree as a practical matter that the difference between basic and applied research is somewhat illusory, the snobbery is too persistent and real to be dismissed by definition. It is not only the result of willful ignorance among academics, although this doubtless plays its part. In my view, the basic-applied tension will always exist, and is actually a creative force for innovation. The tension should be exploited, not denounced. The important thing is that academic-industrial and the academic-Government interactions should be fostered wherever possible, and every effort should be made also to foster a high mobility of scientists between Government, industry, and universities both for short periods and on a long-term basis. My own experience suggests that academic scientists do not shy away from applied work when they see a genuine

opportunity to contribute, and that they have "opposite numbers" in industry and Government with whom they can communicate effectively. Applied scientists, on their side, have a responsibility to reformulate some of their major problems from a basic viewpoint, so that they appear more intellectually rewarding. It is only in this way that the road from science to technology can be smoothed. Applied science in the United States is most lagging in the areas of technology that have fallen so far behind the research frontiers of science and engineering that effective communication between the technologist and the scientist can no longer take place. It may well be that these areas of technology can be reformed only by invasion, by people moving in from relatively remotely related disciplines and seeing the problems and opportunities of an old technology in a new light.

### 9. Judging Applied Research

Quality is very much more difficult to assess in applied science than in fundamental science. If one asks a random sample of scientists who the best people are in their fields, there will be a surprisingly high degree of agreement, but it is much harder to achieve such unanimity in applied science or engineering. Indeed, the scientists in a given area of work are much less likely to know each other or each other's work in applied science than in basic science. This is partly because of the fact that in technology the method of communication is much more by personal contact than by literature. Documentation, especially public documentation, of new ideas, is given much less attention by technologists than by scientists. In tracing the history of innovation, one is struck by the frequency of re-invention of ideas by different groups without knowledge of each other's work. Technology, having a higher component of "art," is much more difficult to codify in formal "literature." Good applied science has to be judged by a mixture of criteria, including criteria internal to science itself but also external to science, in varying proportions, depending somewhat on the closeness of the work to immediate application. In judging applied work, quality has to be defined to a large extent in terms of pertinence to the problem at hand. The further removed one is from problem solving, however, the more important internal scientific criteria become in judging the quality even of applied research.

It is probably important that as much applied research as possible be translated into the "scientific" tradition of public documentation, with as much emphasis as possible on generic solutions. To an extent that is often neglected, this documentation function in the industrial sector is performed by trade journals, which are less formal than the "official" scientific literature but perform an essential function in diffusing new technology throughout an industry.

## 10. Role of the Administrator and the Entrepreneur

The administrator and manager play much more important roles in applied research than in basic research. The administrator of applied research must be more concerned with the substance of the work than is the administrator in basic science, where his job is primarily to assure a proper environment for creativity with relatively little concern for the strategy of the research itself, except as an equal participant. An exception, of course, lies in the management of "big science," where extensive forward planning, and therefore highly sophisticated technical judgment, is required.

The fundamental problem in managing applied research is to provide support that will attract attention and focused effort, but still leave sufficient scope and freedom to reconfigure goals and reformulate problems as the work advances. The good research director seldom manages in the sense of telling people what to do; rather he manages indirectly by showing interest in the lines of research that appear to him most promising and, by himself, persuading people of the desirability and attainability of his own goals. In this respect, there is no substitute for a demonstrated record of past success in enabling a manager to bring his own people along with him intellectually. An important part of the task of a research director is to match science to the non-scientific goals of the organization.

Behind many new developments in technology one finds a dedicated enthusiast, a single-minded individual with a clear and unswerving vision of the goal that he sees as possible and desirable. For rockets, it was Robert Goddard; for transistors, it was William Shockley; for jet engines, it was Frank Whittle; for nuclear-powered ships, it was Hyman Rickover; for hydrogen weapons, it was Edward Teller. In some cases such individuals are swimmers against the tide of respectable scientific opinion, but in others they have the intellectual support if not always the shared enthusiasm of their colleagues. Unfortunately, for every such dedicated individual who is right there are many who are wrong, or even incompetent. In retrospect we tend to recall only those who were right, while the mistakes and misguided enthusiasms or enthusiasts are buried in obscurity. Perhaps the best test in the long run is the ability of such a man to attract other men of high intellectual caliber to his technological banner. Frequently such an individual is not himself the technological innovator, but the individual who, concentrating on a single and simply expressed goal, succeeds in attracting creative people to his own objectives. In this sense, he is a technological entrepreneur.

Little is really known about the characteristics of such technical entrepreneurs, although their importance to technological innovation is evident from almost every case study. They are a distinctive breed that does not fit a mold. Often they are lacking in the orthodox educational quali-

fications, and frequently they may appear scientifically ignorant or non-rigorous. They tend to be eclectic in their interests and tastes, not constrained by the formal boundaries between disciplines.

On the other hand, it would be dangerous to assume that all technical innovation requires such an entrepreneur. In many situations groups of talented people come together to work on new technology without any visible "entrepreneurs" or intellectually dominant individuals. It would be dangerous to assume that enthusiasm is a guarantee of success. The technical entrepreneur is an intellectual gambler who plays for big stakes, whereas the scientist is usually a man who, by temperament, plays for somewhat smaller stakes, but often with surer progress.

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## A HISTORICAL VIEW OF ONE AREA OF APPLIED SCIENCE—METALLURGY

by CYRIL STANLEY SMITH

### Introduction

This essay explores, over a relatively long period of history, the relationship between pure science and technical practice in the field of metallurgy and materials science. Though this is the area of the writer's special scientific and historical competence, it is of general interest because there are few fields of practical endeavor that have had so long and continuous a history or that have been so intimately involved in the development of pure science. Metallurgy is now considered to be an applied science, yet throughout most of history the pure scientist has benefited more from the empirical knowledge of the practical metalworker than the latter has been helped by theory. A comparative view of conditions before and after the reversal of the relationship, which occurred only two or three decades ago, will illuminate the nature of the interaction between pure and applied science.

It is hard to define applied science. Is it the application of already-known scientific principles to practical problems? Or the carrying out of scientific studies of those aspects of nature that are known to be useful—the gathering of data on the behavior of matter in areas of potential use instead of in areas that best promise to illustrate basic intellectual principles? To the extent that it can be distinguished from engineering, applied science can be regarded as a body of experimental methods, experimentally determined constants and theory (not necessarily exact or fundamental) that are likely to prove helpful to engineers and others who have to solve socially determined problems by the use of any available methods and concepts regardless of their intrinsic intellectual interest or lack of it.

As a French minister of education said 80 years ago, "It is sometimes necessary to separate practice and theory because life is short, but they should always be combined when possible because life is complex." It is the vast complexity of materials that has delayed the development of their science. Even now theory can elegantly explain "in principle" only some parts of the behavior of matter, and can say little about the real behavior of matter under complex circumstances. Consequently, the engineer must plan his own empirical tests to select material without detailed

guidance from theory, and he develops useful rules without clear scientific foundation. Moreover, the engineer often finds that even the common physical constants of his materials have not been reported and he must do his own measurements, for the forefront of science has long ago moved away from such simple but necessary determinations as density, expansion coefficient, thermal conductivity, and elasticity, which once were needed to illuminate theory. Very often the engineer uses mathematical methods that at the time of their development were at the forefront of science but which since have become commonplace tools of little intellectual novelty. In a healthy industrial society a reasonable number of intelligent men must be found to work in areas that are neither at an intellectual frontier nor likely to gain popular acclaim and such men will gain stimulation and satisfaction by having an intimate and responsible contact with the two worlds of intellect and of practical accomplishment, and will often receive great material rewards because of the obvious utility of their work.

With notable exceptions, scientists have usually been content to leave application to others; there is perhaps something in the temperament that makes a good scientist that causes him to avoid the makeshift opportunism of a successful industrial development. Yet it also may be that in the past good science really could not be directly relevant to practice in the way that it now patently is. Basic scientists have been quite eager to work on applied problems whenever these have become amenable to their outlook. This is one of the reasons why physicists entered with such zest into many wartime problems. The particular interplay between theory and experiment that was involved in designing radar was not, after all, much different from the activity involved in designing a high-energy accelerator, and it had the added zest of making an immediate and revolutionary contribution to the war effort.

In reading what follows it is well to keep in mind the distinction between the environment in which discovery or initial invention occurs and that surrounding full realization and commercial exploitation. The scientific understanding of a material or a process is of immeasurable value in guiding its development to maximum efficiency, but the very logic of science is likely to close the mind to oddities of actual behavior and, by reasonable success in explaining a phenomenon to deter more profound scrutiny. Similarly, a fairly good technology may remove the incentive for discovering a better one. It should also be remembered that, though history illuminates human nature, its lessons are not easy to apply under conditions that have never existed before. The probability of useful discoveries has been greatly increased by the provision of an environment for systematic studies in selected areas and by the relative willingness of today's industry to seek and adopt changes. In the relatively well-integrated technologies of today areas of ignorance are quickly exposed so that scientific work may be concentrated in them. Neverthe-

less, the role of science is not necessarily made easier to detect because science constitutes much of the education of all technologists and is so intimate a part of their occupational environment.

### The Beginning of Metallurgy

Let us look specifically at the history of some common metals. It is at first surprising to find that nearly all of the general-purpose metallic materials now in common use—copper, brass, bronze, iron, tin, lead, the precious metal alloys and solders—were discovered before 2,000 B.C., well before the period of classical Greece which is commonly taken to mark the beginning of Western civilization, and four thousand years before any scientific theory existed that could possibly have guided their development. Moreover, most alloys and metallurgical principles were first employed in making decorative objects and jewelry, and the decorative glazes employed on early ceramics employed a host of inorganic compounds and used subtle phenomena such as vitrification, crystallization, surface tension equilibrium, and phase relations in inorganic systems that are only now being scientifically explained. It seems that more discoveries arose from aesthetic curiosity and were not prompted by any perceived industrial or military need. Some curious persons must have sought enjoyment in observing the diverse and marvellous effects that result from heating various mixed minerals in the fire, and selected a few successful procedures from a myriad of unsuccessful trials. Though the validation was a sensual aesthetic reaction, not the intellectual logic of today's scientists, the motivation for discovery must have been closely akin to that which provokes discoveries in basic science. A man bent on war or profit would have regarded time spent in indulging such child-like curiosity as wasted. But once a given material had been discovered and its properties displayed, its possible application to other purposes would be easy to see and would provide the incentive for the organization of men and matter that was needed to turn it to profitable use.

Since the common metallurgical materials were discovered so long ago, the metallurgist has, until the present century, been mainly concerned with improving the economics of production. To a large extent, the alloying and use of metals was the concern of the small-scale craftsman—blacksmith, goldsmith, pewterer, tinker, jeweler or foundryman—until the 19th century when the civil engineer began to use in larger quantities whatever the iron-master produced, and introduced tests in order to find what stresses were safe to use in design calculations and to ensure uniformity.

Throughout most of history, the scientist learned from the artisan the wide range of the properties of matter and the manner of its modification and he gave very little in exchange. Robert Boyle was keenly aware

of this, for he wrote in his *Usefulness of Experimental Philosophy*, published in 1671:

For I look upon a good history of trades as one of the best means to give experimental learning both growth and fertility, and like to prove to natural philosophy what a rich compost is to trees, which it mightily helps, both to grow fair and strong and to bear much fruit.

At last, however, science has become able to repay its debt, and the practical arts have changed and flourished as never before, becoming indeed no longer arts but engineering.

### The Growth of a Science of Materials

This story does begin with the Greeks. In establishing a philosophy of matter, some of their philosophers sought elemental qualities beneath the diversity of properties of real materials while others saw true atomicity behind its manifest texture. Both concepts were indeed rightly based, though they were far ahead of any valid experimental confirmation. Yet every metalworker knew that it was easy to change the quality of a material by subjecting it to simple operations—for example, soft iron could be made intensely hard or red copper converted at will into a rich black, a brilliant silver, or a shining golden alloy. The idea of transmutation arose legitimately therefrom. The "elements" of Aristotle—earth, water, air, and fire—do represent energy and the three major states of matter. Had the idea been criticized and refined experimentally by studies of solids, liquids, and gases much could have been learned, but somehow Aristotle's theory seems to have been so satisfactory that it stopped original thought. Transmutation became the goal of the alchemists and the basis of their mystical theories of the microcosm and macrocosm. Mainly because they had too great a confidence in theory, they added little of permanent value to the understanding of matter. It was practical metalworkers doing age-old tasks and especially the better-educated assayers who, by empirical experiment, discovered many classes of chemical reaction and laid the basis for a quantitative chemistry to follow. The scientific explanations of elaborate and effective metal-refining operations lagged two or three centuries behind their use. For example, most of the chemical facts which provide the backbone of the scientific work of the great chemist Boerhaave, early in the 18th century, had been expressed, often quantitatively, in the practical assay books published by Lazarus Ercker and others in the 16th century. The common reactions employed by the assayer and refiner in extracting metals from their ores and separating the various metals from each other provided the basis for the classifications proposed by the theoretical chemist, and suggested problems for research.

Of course, the assayer was narrow-minded and unphilosophical. He greatly overemphasized the precious metals, and he cared little for re-



actions that did not use fire, for he attempted to mimic in the laboratory the operations of the smelter. Commercial motivation, not the search for science, made him weigh his materials and products so carefully. But assaying did eventually become chemical analysis. It was mainly after chemists partially forsook their grand theories and simply undertook the task of determining the composition of matter by analysis that the basis for good theory was laid. The challenge to European ceramic technology that came from the Orient in the form of immeasurably superior Chinese and Japanese porcelain was met by a scientific response after a letter from a Jesuit missionary in 1712 described the techniques and named the principal constituents of porcelain. There followed some intensive studies of rocks of all kinds, and new schemes for chemical analysis arose, particularly in Sweden. This resulted directly in a new and valid definition of a chemical element and the discovery of many new ones.

Long before this the old principles of earth, air, fire, and water had been replaced by Paracelsian salt, sulphur, and mercury—a more realistic array of qualities, for these three materials exemplify three of the four types of interatomic bonds in today's quantum theory of solids. No analytical procedure ever uncovered the postulated principles: eventually the results of analysis made it clear that matter could be divided into two classes, elements, and compounds, and that the latter could be resolved, by suitable laboratory manipulation, into the former. There were found to be far more than three or four elements, but not a limitless number. Metallurgists, with all their millions of fires, had failed to see that fire was both a promoter of reaction in things put in it and also a chemical reaction itself, involving atmospheric air. When metallurgical operations were studied by the new chemistry, the role of atmospheric and other gases was soon found and a great class of substances previously unweighed and largely unnoticed was added to both elements and compounds.

At the end of the 18th century, chemical science was really ready to help the practical metallurgist, who nevertheless up to this point had not been restrained by the lack of it. The scientific developments had occurred mainly in Sweden and in France, but it was in England that virtually all advance in metallurgical operation occurred, as an intimate part and partial progenitor of the Industrial Revolution. First came the successful use of coke in the iron blast furnace by the practical ironmaster, Abraham Darby (1709); then the crucible melting process for tool steel, invented in 1740 by a watchmaker exploiting the newly-appreciated chimney, but fortunately located in Sheffield where suitable fuel and furnace materials were available; and Cort's puddling furnace and rolling mill (1783), which quickly replaced the old finery hearth and forge hammer, and improved the quantity, quality, and economy of wrought iron

production. In not one of these developments did science play the slightest role, but they required a deep intuitive understanding of the nature of iron and a knack of industrial organization. As Derek Price has remarked of similar events, it was technology that bred better technology: the innovation was in technology *per se*, not in science.

English scientists in the 18th century were not idle, but few of them were concerned with the nature of matter. Through the long established iron industries in both France and Sweden were lagging, it was from these countries that the first real contribution of science to iron production came: this was the discovery that carbon was present in steel and cast iron and responsible for the great difference between them. The background of this discovery stemmed directly from practice, and it all started with a stimulus from the Orient. A practical Swedish metallurgist named Rinman was studying the texture that characterizes Damascus steel, and noted that a plumbago-like material was left behind when cast iron was dissolved in acid, but not when wrought iron was. This observation was studied quantitatively by the great chemist Bergman whose resulting theory of the role of charcoal-substance was essentially correct, though overlaid with a fog of phlogiston. In 1786 three great French scientists, Monge, Vandermonde and Berthollet (only the last professionally a chemist), restated the theory in the terminology of the then very new anti-phlogistic chemistry.

It is astonishing to a metallurgist today that any steel or iron could have been made without knowledge of the importance of carbon. All the different techniques of handling iron in the fire that had been built up through the centuries had been selected because unconsciously they controlled the amount of it. Even after carbon had been identified, the knowledge did not immediately lead directly to any changes in technique. The next improvements in siderurgy were the use of the hot blast to improve the efficiency of the blast furnace (1829); then the introduction of pig boiling, a more rapid version of the puddling process employing a cheaper refractory and giving better yields (1839); and then the great steps of producing molten low-carbon "steel"<sup>1</sup> in the Bessemer converter (1856) and in the Siemens open-hearth furnace (1866). The last had clearly a root in science for its inventor based it on considerations of thermal efficiency, though the hot blast had preceded it without science. The background of Bessemer's invention is less clear. Bessemer was essen-

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<sup>1</sup> Steel is in quotation marks because the word "steel" had meant, through all history until the end of the 19th century, only a material of the type used for tools and characterized by being hardenable on quenching. Anything softer was invariably iron. Cast steel was an exceptional product. After Bessemer and Siemens a melted and cast low-carbon metal was for a time called homogeneous iron, but the inflation of words produced by commercial promoters, who had previously turned some brass alloys into bronze, succeeded in turning iron into steel and in so doing obliterated an important distinction.

tially an ingenious and ambitious inventor with little training in science, although he was an inveterate experimenter from youth. In his first published account (in *The Times* [London], August 14, 1856) he says:

I set out with the assumption that crude iron contains about 5 percent of carbon; that carbon cannot exist at a white heat in the presence of oxygen without uniting therewith and producing combustion; that such combustion would proceed with a rapidity dependent on the amount of surface carbon exposed; and, lastly, that the temperature which the metal would acquire would be also dependent on the rapidity with which the oxygen and carbon were made to combine; and consequently that it was only necessary to bring together the oxygen and carbon in such a manner that a vast surface should be exposed to their mutual action, in order to produce a temperature hitherto unattainable in our largest furnaces.

The details set out in Bessemer's autobiography show no such logic; it started rather simply with his observation of an unmelted shell of partly oxidized metal on the surface of a pig of iron in a drafty place in a reverberatory melting furnace with secondary air inlets. His first converters were *not* designed in anticipation that no external heat would be needed, but this fact was discovered during the trials. Moreover, Bessemer's ignorance of metallurgy was such that he did not know one type of pig iron from another, and he was commercially successful with English pig only after he had obtained a good deal of help from leading analytical chemists. Science seems to have contributed even less to the invention of William Kelly, who developed a comparable process in Kentucky a few years before Bessemer. But invention is one thing, development another. There is no question that the rapid growth of the steel industry following Bessemer and Siemens would have been utterly impossible without chemical analysis, without an awareness of the role of the different impurities (mainly silicon, sulphur, phosphorus) and without the leavening influence of the scientific viewpoint of the analytical chemists who joined the staffs of all the large producers of steel.

For a time the chemical composition of materials was overemphasized by scientists. Though composition is important, it is far from being all that is needed to characterize a material. Today's science of materials is based upon physics and consists mainly of the study of structure—how the detailed arrangements of atoms and crystals arise and can be modified, and how desirable or harmful properties are related to the structure or at least dependent upon it. The earliest worker of metal must have used the texture of a fractured piece of metal as a clue to the success or failure of his processes. Probably the Greek and certainly the 17th century atomists were inspired by such observations to think in terms of the particulate nature of matter, but no fertile interaction between practice, empirical experiment and atomic theory arose until well into the 20th century. When (early in the 17th century) atomism was rescued from its Aristotelian eclipse, some remarkably astute guesses as to the relation between the shape and behavior of parts and their gross properties were

promulgated by natural philosophers, but their thinking was purely qualitative and their examples a confused mixture of phenomena that depend on sub-atomic particles, atoms, molecules, sub-crystals, micro-crystals, and cells—differing scales which could not then be distinguished. One man's research is outstanding, that of the French scientist Réaumur (1683–1757) most widely known for his thermometer, though he had a hand in porcelain and his major work was in biology. He had been asked by the French Academy of Sciences to write a descriptive record of the iron and steel industry, and he was so appalled at what he saw that he started a scientific study of it. The resulting book, published in 1722, is a veritable model for an applied scientist's approach even today—studying the existing facts and needs, applying known theory and modifying it as necessary, then planning special experiments to gather data with which to test and refine the theory, and finally to devise improved practical operations. Narrowly missing a clear insight into the role of carbon in steel, he suggested that some material particles, “sulphurs and salts,” went from the charcoal into the iron when it was converted into steel and that an excess produced cast iron. Most interesting, however, is his use of structural models to explain practical operations in the ironworks, as well as the results of his own special experiments. He had absorbed the corpuscular viewpoint mainly from the *Traité de Physique* written in 1671 by the physicist Jacques Rohault, who in turn took much from Descartes. Réaumur studied the changes in the appearance of the fracture of iron and steel in great detail, investigating how the texture changed during cementation and heat treatment at various temperatures and in various ways. He related the observations to his general structural theory and suggested workshop tests based on them. In the course of his studies, Réaumur invented the process of malleableizing cast iron—now a large-scale industry—though he himself failed to make a commercial success of it and soon turned to other interests.

Though Réaumur's work was favorably noted, his methods were not emulated in industry, and because the well-based enthusiasm for Newtonian physics was discouraging Cartesian speculation, scientists eschewed qualitative thoughts about the structure of matter for nearly two centuries. Instead of the rapid growth of structural physics that might have followed Réaumur's work interest virtually ceased, and only in practical observations was the subject kept alive at all.

The chemical side of the picture flourished, however, and by the beginning of the 19th century the earlier advances in analytical chemistry had begun to produce perceptible changes in industry. Analytical chemistry had led to the discovery of new and useful metallic elements. Platinum, the first significant new metal for centuries, was first brought from the New World to Europe as a curiosity about 1740, and it was quickly subjected to experimental study by first-rate chemists. Its alloys were

investigated with a thoroughness far beyond that of any of the long-known alloys. In the same way, the alloys of uranium and of the rare earth metals became the objects of study by the most advanced methods as part of the nuclear energy program in 1940–1950. The process for consolidating platinum (a forerunner of today's advanced powder metallurgy) was a direct outgrowth of scientific study, though it needed industrially-minded men to make and market it. Nickel was discovered in 1751, but it had little use until it was found that *paktong*, a popular white alloy that had been imported from China and was much favored for fireplace furniture and candlesticks (now called German silver, or, after World War I, nickel silver), was an alloy of this metal. The delay between analysis and first attempts at commercial manufacture in Europe was great—1776 to 1823. The alloy became of greater industrial importance when it was found to provide an excellent base for the newly-discovered electroplating of silver in the mid-19th century. Aluminum was long known as a compound but its reduction to the metallic state, possible only after the discovery of voltaic electricity had given other reactive metals, was a scientific triumph.

Most of these discoveries were initially in the realm of pure science rather than applied science. Indeed, the distinction between pure and applied science, if fuzzy today, was much less distinct in the past when the scientist inevitably studied experience to find the range of phenomena and to get hints of qualitative rules before he was able to write equations expressing a law. The empirical experiments of a scientist at this stage are distinguishable from the trials of an alert craftsman only by the thoughts which motivate them. Once good theory exists, however, then it is possible to talk of an applied science. Such a body of knowledge grew rapidly in the 19th century, the most useful to the materials men being the developments in physical chemistry. In the 19th century the beginnings of the study of chemical kinetics were made. The rules of thermochemistry and the study of heat engines led to the principles of thermodynamics, developed mainly in 1874–1878 by the greatest of all American-born scientists, Willard Gibbs. These were applied to alloys about 1900, thereby putting into scientific order a mass of observations on microstructure that had been collected in the previous 15 years by exciting explorations into the new world of microstructure of alloys and were being related to both composition and thermal treatment by the methods of metallography which had become more or less standardized. (There is a fascinating aspect of the interaction between technology and science in the development of the microscope as in all other discovery-promoting instruments, but this must be omitted here for lack of space.) There followed a glorious period in which the determination of phase diagrams was important to both pure and applied scientists—to pure scientists (chemists) because there was a wide field of existence that had to be

explored in the hope of finding the laws of metallic combination; to applied scientists because the resulting diagrams guided the practical man to new alloy compositions and explained old successes and failures. But still the physicist was not involved, until in 1912, came the introduction of X-ray diffraction which enabled the atomic positions to be determined in crystals of metals and other substances. For the first time there was a technique for studying the structure of matter that appealed to the physicist for it was at once atomic and mathematical. A real physics of the solid state then became possible and both the experimental and theoretical aspects of it developed with rapidity.

A second great spurt in the pure science of materials came with the concept of crystal imperfections; first, chemical imperfections in the form of missing or foreign atoms in a crystal lattice, and then the geometric imperfections called dislocations, which by their movement and obstruction are responsible for much of the strength and the weakness of metals. This latter concept began in 1933, but its wide significance was not appreciated until after World War II.

The semi-scientific studies of metallurgists who were motivated by the desire to understand practical operations had prepared the way for a pure physics of solids in much the same way that the empirical smelters and assayers had laid the ground for pure chemical theory in earlier centuries. Again their reward was manifold. In about two decades the detailed information derived from X-ray diffraction combined with the knowledge gained by the older microscopical studies gave sound principles for the development of the new highly-specialized materials without which today's engineering achievements would be impossible. Physics had replaced chemistry as the scientific metallurgy.

As the science changed, though not because of it, the character of people who called themselves metallurgists also changed. Metallurgy had frequently been associated with mining operations, but now the center of gravity of the active science moved toward applications. The training of metallurgists involved much less information on the design and operation of furnaces and much more on solid-state physics. The old "school of mines" man who did so much for a geographically expanding smelting industry has been replaced by the solid-state physicist, who has no feeling for an ore mineral. (Probably the trend has gone too far and one kind of metallurgist will return to a concern, as in the past, with economical primary production of metals. Actually, throughout most of history, it has been the user—the armorer, locksmith or jeweller—who has been most concerned with the final properties of alloys, and it is not illogical that the knowledge that makes complex uses possible should be differentiated from that involved in the chemical and mechanical stages of primary production. The oddity is that the same name, metallurgist,

should have been stretched to cover what has become essentially a new profession.)

The scientist likes to work with the simplest possible manifestation of any phenomenon, for only thus can he achieve rigor, yet he may not know that there is any phenomenon to be interested in until it has been revealed by the peculiar behavior of some complicated system. Studies of steel, one of the most complicated metallic materials, first revealed that there were phase transformations (internal crystal structure changes) in solids. Even the metallurgist's study of grain size, which could best have been done with well-behaved copper, began with steel. This was, of course, mainly because of the economic importance of steel, but it also may be that the temperament of applied scientists leads them to a delight in complicated things that would repel a man of more stringent intellect. However, once techniques became available to relate macroscopic behavior to models on an atomic scale—as by X-ray diffraction—the pure scientists's interest is immediately aroused.

An interesting case is development of the ductile tungsten filament so essential to the development of electric lighting. This was not the achievement of a metallurgist but was done in 1908–1909 by W. D. Coolidge, a first-rate chemist employed by industry. The techniques that he developed were effected because they controlled grain size and shape. But theory did not lead to the discovery—quite the contrary—for once ductile tungsten had been made, then metallurgists studied it intensively and developed therefrom the general principles of grain growth which thereafter were applied to all metals, including those that had been used for centuries. Common rules are often discovered from the behavior of exotic substances. Unless interest is at first attracted, understanding will not come about.

Another example is the development of the solid-state physicist's interest in imperfections in crystals, stemming largely from the curious behavior of semi-conductors, the properties which were employed in radio and radar somewhat before a good theory existed.

Perhaps the greatest impulse toward the development of new materials, and to a more profound understanding of the old ones, came about with the development of sophisticated industries that used materials. Consider the case of heat-treated alloy steels, which are so superior to the best earlier steels that they constitute virtually a new class of material. They are essentially a 20th-century development.

The producers of both non-ferrous metals and steel naturally devoted their energies and ingenuity to the production at low cost of large quantities of a few standard materials. The training of metallurgists before 1930 was almost entirely aimed at service to producing rather than consuming industries. Some experiments on alloy steels had been done early in the 19th century but interest in them grew slowly. A tungsten alloy

tool steel was marketed before 1870. Chromium steels were tried with indifferent success in the St. Louis Bridge, 1870-1874. Manganese steel was demonstrated in 1882 but not used commercially until ten years later. The first widespread use of an alloy steel was that of the nickel steel pioneered as naval armament around 1890 as a result of the aggressiveness of a producer of nickel seeking a market. This material was not heat treated. The real impulse to the development of heat-treated alloy steels came with the opening of the 20th century and the automobile; the manufacturers saw that different components needed steels having widely different characteristics and they were willing to pay for specialty.

Even more important in determining the new attitude toward materials generally was the growth of the electrical industry, for it emphasized properties other than the ancient mechanical requirements of strength, hardness, formability, and resistance to corrosion. Scientific studies of the relationship between conductivity and alloy constitution were given a great fillip by the first trans-Atlantic cable in 1856—which, incidentally, was two-thirds finished before anyone thought of specifying the conductivity of the copper wire, and some, containing arsenic, had a conductivity of only 14 percent of the purest copper then available. The copper in the 1865 cable had a conductivity of 96 percent, and the man mostly responsible for the development, a chemist named Matthiessen, had meanwhile started some important studies of the characteristics of different kinds of alloy systems.

The first big improvement in magnetic properties of iron for electrical transformers came from the cooperation of an aggressive metallurgist and a physicist, though the observation of the unusual magnetic properties of silicon-iron alloys was totally unexpected, not a prediction of theory. The next development in transformer iron was the production of silicon iron in a form in which the crystals were arranged in preferred directions. It was a most important advance, but it was guided by an erroneous theory supported by a gross misinterpretation of experimental results! It does not matter if theories are wrong—indeed, they are never completely right—for anything that incites an active search and decreases its randomness will improve the efficiency of discovery and will lead eventually to better understanding provided that the researcher is sufficiently alert and is not blinded by over-attachment to his original hypothesis.

Long before the days of electronics the electrical industry needed a host of special materials—conductors, resistors and insulators, materials for service at high temperatures, friction and anti-friction materials, carbon in special form for brushes and commutators, magnetic materials both hard and soft. In all cases what was needed was a property or combination of properties, and it was immaterial what material possessed them, or by what techniques they were shaped provided they were rea-



sonably cheap, and even this last economic factor was strongly modified by the great economies that could accrue from the exactly adapted material in the right place. How different this attitude was from that of the producers of the standard metals, who were forced by short-range economics into cheap quantity production. Because the oldest materials are mainly those of great versatility, doing many things fairly well but none superlatively, there is a rather direct relationship between the youth of an industry and its receptiveness to science. The aluminum industry, starting from scratch in the last decade of the 19th century, used science intensively to shorten the search for alloys to compete with those that had been known for millennia. It is the consuming industries that took a comparative view of all materials and techniques of production; in a generally scientific framework but with stringent service requirements in mind, this I believe, has been responsible more than any single factor for the changed attitude toward materials in the last few decades. No longer does the advanced engineer simply select materials from those already on the market, but in collaboration with a materials engineer he designs a material as he designs a structure, each influencing the other for optimum conditions. The materials scientists is at the forefront of development, participating directly in it, not simply producing something for an unknown use. He not only "applies" science, he develops areas of science that are needed for the purposes he has in mind. Just as industry has come to look at properties rather than composition, so the materials engineer himself has come to study the science of all materials. Whereas once even the ferrous and nonferrous metallurgists were somewhat separated, slightly in college and completely in employment, nowadays the emphasis is upon understanding the structural basis of all things. In college metallurgy has become materials science with a very profound change of outlook and with studies of metals, semi-conductors, ceramics, and polymeric plastics all incorporated into one framework.

The basic knowledge of the pure scientist keeps its value and is worth doing regardless of its immediate relevance to other sciences or its value to industry. Much applied science, however, is in areas of such complexity that it is of fullest value only in the complex circumstances for which it was intended, and if the circumstances change its value decreases drastically. Applied science cannot be done in a vacuum. Well-planned applied science often consists in finding the detailed application of known principles to the understanding of a wider and wider variety of systems. Much of the excitement of applied science comes from recognizing in a real but complex system a well-understood general principle as well as in the anticipation of finding hints of previously unknown classes of theory.

One of the greatest difficulties in the study of history is to avoid the transfer to earlier times of today's standards and points of view. In one

sense science has existed as long as homo sapiens; in another it is very recent. Anything approximating the extent of understanding of the natural world that we have today is a 20th-century phenomenon. Few scientists believe much in the "scientific method" of which philosophers are so fond. Rather science is to be regarded simply as a somewhat disciplined curiosity about the world; an attitude of mind, a mixture of observation, deduction, planned critical experiment, and induction, aimed at developing concepts of logical structure possessing widespread applicability. But on creative occasions this must be combined with a willingness to throw aside all rules and to use intellect or emotion, mathematics or hunch, or any other inciters of insight. *After* the moment of discovery there comes validation by fellow scientists who will accept only the result of experiment or logic, and the consolidation of the theory into the whole framework of knowledge which both reinforces it and is reinforced by it. The whole structure then provides useful rules that can be applied even by relatively unimaginative people to areas that have been passed by, and at the same time the discrepancies between the structure and actuality continue to provide the incentive for its improvement.

There is a limit to the simple and to the conscious combinations of the simple. The metallurgist or engineer intuitively combines physics with many unknown factors. There is a fuzziness in the application of all definite knowledge to reality, which is necessary to allow for complex factors in combination. The artisan and the artist and their modern counterparts in industry have an uncommunicable understanding of this. The life of an applied scientist is an exciting one, for he is surrounded by innumerable entrancing phenomena; if he lacks the supreme insights that the fundamental scientist gains into the ultimate nature of the world, he at least has closer contact with it on the human scale, and in understanding its complexity has many of the satisfactions of the artist. The broadening and deepening of the body of knowledge obtained by science has been achieved mainly by an increasing ability to express the results precisely in mathematical terms. This very precision has excluded some aspects of the real world, and qualitative observation and classification will doubtless always play a role in preparing the ground for more exact treatment. Few scientists will deny that their most exciting moments are those of intuitive insight rather than mathematical proof.

It seems that the best way of fostering applied science is simply to give a good basic scientific education to those students who plan a career in industrial research, but to do this without killing their interest in more complicated areas of science and technology, those less amenable to exact treatment. Research is an extremely important component in this educational process for it provides the student with the opportunity to see that both science and engineering are not only firmly established knowledge,

but also consist in a living experience. It is, therefore, especially important to support university research in the applied sciences. The training of students is even more important than the factual knowledge that results from the research they do, for the future depends upon it.

To a large extent the accessible frontiers of fundamental science have been conquered and the remaining ones are in a terrain that is incredibly hard to reach. This, combined with the fact that science previously ignored by the public, is now seen to be important, must result in a profound change in the environment of the scientist and the very nature of science itself. Less absolutely pioneering, scientists will work more in filling in the areas that are already understood in austere principle but not in rich detail, and as they do this they will inevitably be concerned with more and more complicated systems. For economy's sake they must mostly be concerned with such systems as already exist or which are to be established for some social reason. If science does become more complex in the future, then it seems likely that the separation between the pure and the applied scientist will be less obvious than it has been in the past.

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## THE SYSTEMS APPROACH

by HENDRICK W. BODE

### Introduction

This paper is intended as an essay on systems engineering, regarded as one aspect of "applied research." Many of the problems confronting the nation, such as urban sprawl and decay, transportation, air pollution, water supply, and so on, involve large systems, and the "systems approach" seems a natural way of attacking them. Systems engineering may also have value for other, smaller, issues, and is frequently urged in such connections also. There seems little doubt that this approach may help us on many fronts. On the other hand, the "systems approach" is sometimes urged, somewhat uncritically, as a sovereign nostrum for all ills. Thus one needs to pay particular attention to aspects of the subject that may make it appropriate or rewarding in some situations but not in others.

Complex situations involving systems-engineering considerations are by no means new. Regional development plans of the past, such as the TVA evidently involved such considerations, just as much as do the environmental projects of the present day. They were, of course, taken care of somehow, whether or not they were identified as systems problems. On a smaller scale, such typical problems as inventory management or the proper scheduling of railroad rolling stock or university classrooms have been with us for a long time, and these must also have been solved, if not always with maximum efficiency.

As a formal discipline, operations research, which can be regarded as a first cousin of systems engineering, developed during the war, especially in connection with bombing and antisubmarine problems. It has flourished ever since, in both military and nonmilitary contexts. Inventory and scheduling problems, for example, would now be undertaken under this heading. In the private sector of the economy, the Bell system has used its own version of systems engineering for many years as a means of planning the growth and extension of communication facilities in an orderly way. In this special area, there is a vast background of relevant experience to draw on. Many of the techniques of present-day systems engineering, such as those related to traffic and queuing, turn out to have been first developed in connection with telephone problems. Some of this experience will be touched on later.

In recent years, the most conspicuous examples of the systems approach have been in connection with large military and space projects. In these applications the "systems approach" may be identified with the initial planning of the project, but it is equally likely to refer to a method of managing a complex technological development so that it can go forward more or less according to a prescribed schedule. In either case, the prestige of the approach is very great. It will be remembered, for example, that the Air Force Research and Development Command, one of the major divisions of the Air Force, was renamed the Air Force Systems Command to give added emphasis to its operations.

Associated with these activities a fairly elaborate formal methodology has been developed. One has only to think of linear and nonlinear programming, game theory, decision theory, queuing theory, and so on, as examples. In general, the techniques tend to be highly statistical or probabilistic and to make great use of computers. A simple but important example of the latter sort is the direct simulation of a complex physical system on a high-speed computer to permit the system response to be explored over a wide range of conditions. It is essentially these techniques, associated with the aerospace industry and related parts of the electronics industry, that are usually thought of when the "systems approach" is recommended for other applications in our technology.

### Some Misgivings

It is simplest to begin by admitting at once that, in the long run, the protagonists of the "systems approach" for other than military applications are no doubt right. There must be many areas, like telephony, in which systems engineering will be useful. On the other hand, it takes only a little reflection to convince oneself also that techniques and methods of thought developed in the aerospace world may not always be immediately appropriate in other contexts. There are simply too many differences between that world and others.

As one example, in military equipment very small margins of performance are sometimes decisive. Thus balances between performance and cost may be quite different from those we would find in other situations. As a second example, considerations of military urgency have only faint counterparts in most civilian situations. This is, of course, important whenever cost-time trade-offs are involved, and is particularly relevant if we try to interpret the "systems approach" as a development-management technique. Finally, although requirements for compatibility do occur, when weapon systems are redesigned it is fairly likely that they can be redesigned from the ground up, with comparatively little regard to what has gone before. They need to be, if maximum performance is to be achieved. In contrast, most of the proposed large-scale civilian

systems will probably require plans that will salvage as much as possible of existing equipment and facilities. The design engineer will find himself circumscribed accordingly. Telephone systems engineering has long faced a similar problem in the requirement that any new telephone system must be compatible with all existing equipment in the telephone network. In this respect, as well as in respect to the performance, schedule, and cost considerations mentioned earlier, telephone engineering seems to be closer than most military systems work to the general engineering style required for the putative systems applications of the future.

The impression that the gap between military systems engineering and proposed civilian applications may be greater than most people realize is heightened as soon as one inspects the substantive material with which the systems engineer must actually deal. Thus, one has only to pick up a currently accepted handbook on systems engineering to see how heavily it is larded with reference chapters on such topics as the physics of the atmosphere and outer space, the techniques of telemetry and radar, rocket propulsion, and so forth. The material seems to be well chosen for the purpose in hand, but a similar factual background for an urban reconstruction project, for example, would obviously be quite different. Without going further, one would expect that the urban project would require much stronger inputs from the social sciences. Since social and physical scientists tend to have somewhat different intellectual styles, this might not be entirely easy.

One may also have misgivings of a different sort. Many of the tenets of systems engineering seem pretty obvious and unlikely to contribute anything that would not have been discovered anyway. For example, the systems approach sometimes reduces to a pedestrian enumeration of all possible logical cases. This may occur, for example, in the development of checkout procedures for a missile, or the enumeration of possible failure modes in a space vehicle. As important as such lists may be, they seem to require little more than a decision to do a careful and thorough engineering job. We might have made such a decision without ever having heard of systems engineering.

Another example is furnished by the standard systems-engineering principle that systems effectiveness must be judged with all relevant considerations in mind. Thus, a householder or factory owner may be said to be using the systems approach if he buys electric lights on the basis not only of their initial cost but also of their efficiency in producing light, and still more so if he includes cost of replacement, as measured by expected lifetime, especially for the hard-to-get-at places. (Premium-priced bulbs are available for such purposes.) This is indeed an example of the systems approach, but one might hope to get the same results by the application of ordinary common sense. Moliere's character was impressed by the discovery that he had been speaking prose all his life.

Scientists and engineers are less likely to be bowled over by the discovery that they have been practicing systems engineering all this time.

It would be possible to extend this list, but the examples given should be enough to show why one may be pardoned for doubting whether the systems approach is automatic magic, guaranteed to resolve all our most complicated and intractable problems. On the other hand, the examples do not necessarily refute the approach either. The fact that some aspects of systems engineering seem so obvious does not necessarily make the method logically invalid.

Clearly, we must dig deeper to find the distinctive contributions that systems engineering as now understood should be able to make it the areas that now trouble us. The examination of this question is the central topic underlying this paper. Unfortunately, a short paper cannot give very conclusive answers for such a broad question. Too much may depend on individual circumstances. The objective of the paper will be only to describe systems engineering in a sort of broad average sense, leaving questions of specific applicability to be explored as they arise. For this purpose I shall draw on such background as I have in the military field, but the principal source will be Bell System experience, which for the reasons noted earlier seems in some ways more relevant to the country's new problems.

### Varying Conceptions of Systems Engineering

It seems natural to begin the discussion with an immediate formal definition of systems engineering. However, systems engineering is an amorphous, slippery subject that does not lend itself well to such formal, didactic treatment. One does much better with a broader, more loose-jointed approach. Some writers have, in fact, side-stepped the issue entirely by saying simply that systems engineering is "what systems engineers do." In this paper, also, I shall attempt to define systems engineering only indirectly, through a description of some of its principal characteristics.

The most fundamental reason for the slipperiness of systems engineering is probably the fact that the term does not actually mean the same thing in all situations or to all people. Thus there is some built-in "conceptual rubber" that must be identified before we can go ahead with a more specific description of systems engineering along the lines developed later in the paper. To introduce this preliminary discussion here will mean retracing some of the same ground later on, but it seems important to deal with some of the major sources of ambiguity as quickly as possible.

A look at "what systems engineers do," and the intellectual resources they draw on to do it, suggests that there are three important issues

that particularly affect the character of the subject. They will be identified in this discussion through the catch phrases, "methodology or technical substance," "early or late," and "staff or line." The first of these was adumbrated in the preceding section. It can be understood by reference to operations research, introduced earlier as a sort of first cousin of systems engineering. Operations research can be described roughly as a study of the best way of making use of existing resources or equipment. It grew up during the war, when for obvious military reasons one carried on as best he could with what he had. Thus, typical questions during the war concerned the best employment of a given total number of bombers, the best ways for arranging convoys with a given inventory of vessels, and so on.

In most cases, adequate substantive knowledge of the properties of the equipment was rather easy to obtain. Thus, the principal assets of the operations analysts have traditionally been methodological. A large fraction of the methodology referred to in an earlier section was in fact developed originally in an operations-research framework. (One hastens to add that "methodological assets" must be interpreted to include experience and an acute mind. For example, Morse and Kimball (1) stress the advantages of a background of scientific research in fostering the ability to pose the right issues.) The operations analyst is thus somewhat in the position of a lawyer who can make a good logical case without knowing a great deal about the substantive aspects of the problem, simply by drawing on a vast background of legal knowledge and experience.

Systems engineering, on the contrary, is frequently described as being like operations research, except that the systems engineer deals with new rather than with existing equipment. In principle, this makes the systems engineer into a design engineer with a somewhat special set of tools. One should visualize the systems engineer as formulating a tentative design for the new equipment, studying it by the methods of operations research, using the results to formulate a new tentative design, and so on. If the operations analyst resembles a lawyer, the systems engineer resembles an architect, who must generally have adequate substantive knowledge of building materials, construction methods, and so on, to ply his trade.

Whether or not this is more than hair-splitting depends on circumstances. There is a continuous spectrum of possibilities. At one end, the issue is of little account. Thus, it would not matter to an operations analyst in a commercial situation whether given equipment was literally in his possession or merely commercially available, so long as its properties were adequately known. The systems engineer is in almost the same situation when he specifies apparatus that may be new but that can readily be constructed by known methods, and with easily predicted



final characteristics. The situation is different if the apparatus to be designed is really new either in the sense that it may require the application of new physical principles or that it requires an extension of the state of the art beyond the limits of familiar experience. The physical uncertainties in the situation, or the difficulties of translating theoretically obtainable design advances into quantifiable operational gains must now engross the systems engineer's attention. His job becomes very demanding. An understanding of operations-research methodology may still be important, but unless it is married to a strong base of knowledge of the substantive technology the results are not likely to be optimum.

The proposed new applications of systems engineering can be expected to show many variations in the balance between methodology and substantive knowledge. Thus this is almost the first question one should ask in examining a new situation. Physical scientists, at least, should be especially wary of situations that may involve substantive questions in the social sciences. In the writer's view, the largest and most complicated projects are particularly those that are likely to raise important substantive questions. This is partly just because they *are* large and multifaceted and partly because any project involving a large investment and lasting over a long time deserves special attention to make certain that it has the best tools and equipment available. Thus the rest of the paper will be biased toward such situations.

The issues identified by the other two catch phrases represent other aspects of the general question, "What do systems engineers do?" Thus the phrase, "early or late," refers to the stage in the design process in which the systems engineer is supposed to be most active.

In one concept of systems engineering, the systems engineer is most active in the very early stages of the design, where the primary questions are physical feasibility, the over-all design concept, and estimates of ultimate values and costs. In another concept, the systems engineer is most active very late in the design process. It is assumed, in effect, that the physical apparatus has already been designed, and it is the job of the systems engineer to prepare operating instructions, test procedures, maintenance instructions, spare-parts routines, and so forth, to make sure that the "system" will actually operate satisfactorily in practice. There is also an intermediate concept in which it is supposed that the over-all system has been fixed, and that the job of the systems engineer is to provide detailed specifications for subsystems, involving an appropriate balance between cost and performance for these individual components.

All these functions are important and a complete development might need all of them, although they would probably not be performed by the same people. However, this discussion will consider only the early and intermediate stages. The "late" stage seems less critical, both because it cannot come into being at all unless the first two stages are passed and

because, as demonstrated in experience, extra effort here cannot fully compensate for bad work at earlier points. On the other hand, sloppy handling of the late stage can negate the results of good work in the early stages. Also it is often important that considerations from the late stage be factored into the planning in the early stages.

The third piece of "conceptual rubber" was identified by the catch phrase "staff or line." It refers to the basic organizational position and function of the systems engineer. More broadly, it is supposed to raise the question of the extent to which essentially management activity, as opposed to purely technical work, is taken to be part of "what systems engineers do."

It is convenient to return once more to the simpler case of operations research. Here we find that the basic conception is clearly that of a staff function (2). The operations-research groups are expected to be small and informal, to report directly to the executive, and to carry no line responsibilities. Since the groups do report to an operating executive, however, and since it is easy to add such specialties as decision theory to the methodological arsenal, the over-all "management flavor" may be quite high. In consequence, operations research is sometimes classified as one of the management sciences.

In systems engineering generally, the situation is more complex. In some cases the systems-engineering groups may be like the operations-research groups just described. In other cases, however, they turn out to be larger and more formally organized, with a character more nearly like that of line organizations themselves. In such cases there may be a variety of patterns for the interaction of the systems areas with other parts of the total establishment. The organizational interplay in the Bell System, as one example, is described later.

In military areas, the drift of systems-engineering groups toward something approaching a line responsibility was stimulated by the military's need to prosecute relatively high-risk technical programs in an efficient and schedule-conscious way. The systems engineers, who had conceived the program in the first place and understood the trade-offs among various performance characteristics, could be expected to be the most resourceful people in dealing with unexpected technical problems as they arose. Thus systems-engineering groups were enlarged and held more directly responsible for final results. The phrase, "systems engineering and technical direction," to describe this enlarged responsibility, especially where a number of contractors are involved, has become common. Specialized project offices, staffed largely by systems engineers and involving a complicated matrix overlay on existing functional organizations, are also common. As a result of this evolution, systems engineering is sometimes regarded as primarily a management technique, suited to keep a complicated program on schedule and budgetary rails. In this

interpretation, it thus rests as much on PERT<sup>1</sup> charts and similar devices as on the more classical methodologies described previously.

Management questions of this sort are not considered further in this paper, which is directed primarily to the purely technical aspects of systems engineering. It is important, however, that the issue be canvassed whenever it is proposed that systems engineering be introduced into a new situation. Systems engineers best suited for a project-management role are not necessarily those best suited for purely technical responsibility, and conversely. It is particularly important that the over-all management and organization plan be thought through clearly in advance. To introduce a systems-engineering group into a situation in which these issues are muddy is to invite trouble.

The analogy between the systems engineer and the architect suggests one more point that may be worth making. We have thus far approached systems engineering as though it could be understood entirely by criteria appropriate for any other branch of engineering, or perhaps management. But, like architecture, systems engineering is in some ways an art as well as a branch of engineering. Thus, aesthetic criteria are appropriate for it also. For example, such essentially aesthetic ideas as balance, proportion, proper relation of means to ends, and economy of means are all relevant in a systems-engineering discussion. Many of these ideas develop best through experience. They are among the reasons why an exact definition of systems engineering is so elusive.

### Systems Engineering in the Over-All Engineering Structure

These diverse interpretations of the term, "systems engineering," suggest many variations in the way in which systems engineering enters the over-all technological and engineering structure. The complete technological process, as far as engineers are concerned, is classically divided into the three stages of research, development, and production. In this classical division, of course, systems engineering does not appear at all. In some of the concepts just outlined it might be represented by a box interpolated between research and development or perhaps after production (and before final use).

In most cases, however, such a scheme is too simple. In the first place, the modern technological process is much more complex than the tripartite classification would suggest, even if we did not have systems engineering to deal with. For example, the Department of Defense breaks up the heading, "development," into three subheadings—"exploratory development," "advanced development," and "engineering development"—in drawing up its accounts. Other people use slightly different words, but the terminology is just as complex. In addition, the actual

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<sup>1</sup> Process Evaluation and Review Techniques.

phenomena do not correspond to a simple linear progression from basic research to production and use. There are various sorts of interactions and feedbacks that may be of great importance for the end result. They are likely to be of particular interest to the systems engineer if parallel paths or interactions between non-consecutive stages are involved. To represent such relations adequately, however, we must think of systems engineering itself as being outside the main structure.

One is reminded of the tripartite division of society into upper, middle, and lower classes with comparatively tight boundaries, in conventional sociology. Contemporary sociologists, of course, feel that they need at least five or six classes to do justice to our world, and that they still must allow for a great deal of social mobility among them. Even so, there are individuals who do not fit well into the scheme; intellectuals and union leaders are the sociologists' favorite examples.

In a similar way, systems engineering can be thought of as having a position somewhat off to one side of the principal engineering structure, but with connections to the main line anywhere from the research area to the final consumer. This is essentially the concept described later for the Bell System. Thus, what we have in view is in some sense an over-all monitoring and/or control function for the complete path from research to final consumption. Whether or not such a function is needed, and the exact form it takes, must, of course, depend on circumstances. There may be no need for it at all if the main development path is sufficiently short and direct. On the other hand, the function becomes increasingly necessary if the path is long and indirect, if it is actually composed of a number of intertwined development paths in parallel, or if there is danger that the ultimate needs of the consumer will be forgotten during the development. From this point of view, the emergence of systems engineering as an important discipline is a natural consequence of the increasing power and complexity of our technology and the fact that we are more and more willing to attempt ambitious, long-drawn-out projects.

### Characteristics of Systems Engineering

We can now proceed with a more specific description of systems engineering by listing some of its most common characteristics. The list is taken from a variety of experiences. It is supposed to describe systems engineering in some sort of average sense if we limit the subject in the ways implied by the discussion just finished. As will be seen, the characteristics range from the near-platitudinous to the fairly debatable.

The characteristics of systems engineering that seem to be most important for the present discussion are the following:

1. To begin with, systems engineering is normally concerned with complex aggregates of simpler components that interact to produce a de-

sired result. Examples are the roads, dams, electric generators, transmission lines, and so forth, that make up the TVA system, or the transmission, switching, and station equipment that makes up the telephone network. It is important that there be significant interaction among the components, at least to the extent that they compete for the same dollar support, since otherwise there would be nothing for the systems engineer to do. He could immediately turn to simpler subsystems. It is also important that the components be essentially under design control, since otherwise again there would be nothing for the systems engineer to do.

The systems engineer's first job is to formulate an over-all systems concept essentially a creative step which governs the relations among the parts in a broad sense and can be taken as the first stage of the over-all design process. Unless the situation is unusually straightforward, of course, a finally acceptable systems concept is likely to be the result of much study and many trials.

The analogy here with the architect's job in planning a new building is obvious. The architect must somehow produce a design concept appropriate to the site, functions, and permissible cost of the building. The various functions that must be accommodated in the building interact strongly with one another at this stage. Later on specific subsystems, like heating, plumbing, and so forth, can be separated out and treated independently. If the architect's job is well done, the final building will have a definite unity and style of its own. It will not, for example, be half Georgian and half modern.

2. The immediate objective of systems engineering is generally taken to be optimizing the output, according to some given criterion, by trade-offs among the performance characteristics and costs of the individual components. A great deal of the methodology of the subject is concerned with means of making optimization studies. Experience suggests that exact optimizations of this type are likely to be of little direct value. The assumptions on which they are made are too uncertain, or, in any case, the maxima are so broad that the optimum is not critical. The indirect value of such optimization studies, however, may be very great. By performing such computations over and over one gradually gains insight into the considerations that are really decisive, and evolves mathematical models that are accurate enough for the purpose, and yet simple enough to be computable. Particularly sensitive assumptions or parameters should, of course, be given special attention. Thus, such computations are likely to be an essential step in formulating an over-all systems concept of the sort described under (1). We must expect, of course, that the over-all concept may always be subject to feedback from later stages of the study, and cannot be finally crystallized until the whole situation is thoroughly understood.

3. The problem of computing optima is frequently complicated by the fact that the final system may have to meet several logically independent criteria. For example, a new telephone system may be judged by the criteria of cost, quality of the service, flexibility for future growth, and so forth. The TVA system was required to serve a variety of objectives—flood control, power, navigation, improvement in regional living patterns—with the same physical installations. The architect designing a house may have to consider a number of criteria—adequate bedroom space, easy heating, an impressive facade, and so forth.

A balance among these considerations cannot logically be struck by the architect or systems engineer acting alone. He must consult the wishes of the ultimate user of the system. What the systems engineer or the architect *can* do is elucidate the problem by exposing such basic conflicts, by making illustrative computations to exhibit the trade-offs between various characteristics, and so on. In complicated situations, this is one of the most important responsibilities of the systems engineer. He must do his best to see that such issues are thoroughly understood before final commitments are made.

4. In most systems engineering, the statement of the conditions under which the response of the system is to be optimized may also be an important and difficult question. In many complicated situations the inputs are not quite determinate, but are, instead, statistical or stochastic in nature. The random incidence of telephone calls on the telephone system, of enemy intruders in an air-defense system, or of rainfall in the Tennessee Valley are examples. In the case of the telephone system one has to reckon with the predictable heavy calling rates on Mother's Day or Christmas, in the case of the Tennessee Valley with an occasional wet spring, in the case of the air-defense system with the possibility that the enemy may concentrate in one particular area. In fact, in military systems generally one must reckon with the consequences of deliberate efforts on the part of the enemy to negate the system. One must, of course, also allow in these calculations for possible failure or damage modes. Thus, an important responsibility of the systems engineer is to prepare the material so that the weight of all these considerations can be correctly assessed.

5. Many of our present-day systems are so complex that they may take many years to implement. Since we are also living in an age in which science and technology advance with unprecedented rapidity, there is a very great possibility that the system will be unnecessarily behind the times when it is finally built. This is one of the most important as well as one of the most difficult problems that confronts the systems engineer in planning ambitious new systems. On the other hand, unless he discounts the future to some extent, he will emerge with an unnecessarily poor result. On the other hand, such discounts clearly carry with them

substantial elements of risk. To deal adequately with such problems the systems engineer must have, or be able to call on, exceptionally sound scientific and technical judgment on possible advances in the state of the art in relevant areas, and he must be exceptionally diligent in planning fall-back positions in case the first hopes are disappointed, or in planning ways of exploiting unusually favorable technical advances if these should occur. These considerations provide perhaps the principal justification for extending the systems engineer's responsibility from initial planning into some cognizance over participation in the actual management of the project as it goes on.

6. Closely related to this last consideration is another. Many complex projects in weapon systems, regional development, and so on, naturally take place in a series of stages or "bites." The first stage is often the only one that can be planned for concretely, but one must assume that subsequent stages will, in fact, follow. Obviously, the systems engineering for the first stage ought to leave us in as flexible a position as possible for the later stages.

7. One frequently hears it said that systems engineering always deals with interdisciplinary problems. It is true, of course, that large systems projects frequently require contributions from many disciplines, simply because they are so large and multifaceted. It is the business of the systems engineer to recognize such situations and make certain that necessary inputs are secured. However, to make the interdisciplinary character the decisive elements is to miss the point. Systems-engineering problems still occur in comparatively homogeneous environments, such as telephone engineering. Moreover, a satisfactory system is usually not just a composite of heterogeneous elements, any more than a cake is just some flour, some sugar, a few eggs, some milk, butter, and so on. The ingredients still have to be mixed, and the cake must be baked. The baking process is an essential.

One frequently sees interdisciplinary conferences break up in a state of euphoria. Everyone observes that his own discipline is logically relevant, and hopes to make a contribution without extending himself. Except possibly for the very early stages of an investigation, I believe that such hopes are doomed to disappointment. At some point a greater element of intellectual continuity must enter. It seems to be unhappily true that real intellectual progress in any field calls in time for prolonged hard work.

8. Finally, systems engineering is distinguished by the persistence with which it turns to the needs of the ultimate user as the final criterion for planning. Thus, in the initial stages the necessity of establishing that there is a need worth satisfying, and that we know quite accurately what this need is, dominates one's thinking. Later on, the systems engineer must be the conscience of the project, to make sure that immediate en-

grossmen with the details of the work does not divert attention from its ultimate objective. Here, again, the parallel with the architect who must understand the real requirements of his clients before he can design a satisfactory house for them is obvious.

### Systems Engineering and the Bell System

To make these observations somewhat more concrete, it is worth while to turn to the special case of the Bell System. Within the Bell System, systems engineering is practiced primarily in Bell Telephone Laboratories. The systems organizations there, working primarily with counterparts in the American Telephone and Telegraph Company proper, assess the present and future needs of the operating system, and from that generate plans for engineering developments to meet them. These activities take up about 10 or 15 percent of the professional manpower of Bell Telephone Laboratories, which is about the same percentage as one finds in the research area proper. Engineering development at various levels, plus related support activities, take up the rest, which is clearly the lion's share of the total. Although these proportions have been arrived at principally by experience, they seem to represent a reasonable balance of effort.

Systems engineering in the Bell System provides a relatively good illustration of the field because the objectives of the activity are comparatively clear and obvious, and because work of this general nature has been carried on for a very long time, so that work patterns are well shaken down. An anticipation of the present concept of systems engineering can, in fact, be traced as far back as 1907, when a predecessor organization to the present Bell Laboratories was established. The situation can be described by quoting a statement about the 1907 organization written by one of the executives of the American Telephone and Telegraph Company some years later:

The reorganization in 1907 consisted of a consolidation (whose) purpose was to avoid duplication of facilities as well as to get the greater efficiency coming from a closer contact between the staff of the Western Electric Company and our own. (He means the A.T. & T. Company.) It brought to one point scientific study and research, manufacturing experience and operating experience. . . . It simplified and expedited the work of the operating (telephone) companies in that it established one point where all statements of requirements, suggestions of improvements, or criticisms arising out of their operating experience could be considered and discussed from all points of view. . . . It was helpful in the standardization of apparatus . . .

We had at Bell Laboratories the scientists whose work involved laboratory facilities, the men conducting experiments, the shop design workers and the inspectors with suitable equipment of laboratories and model shops available for all. The ideas of our research and development scientists and engineers, worked out on paper or in rough mechanical form, were there developed into a finished piece for shop manufacture and after manufacture the product was then subjected to all the tests necessary to satisfy our engineers that it was worthy of introduction into or continuation in the plant of the Bell System.



Most of this excerpt is concerned with the advantages of an integrated organization in a broad sense. The systems engineer as one of the means of holding such an organization together is not mentioned explicitly. He would hardly have been needed in an organization of the size and relative simplicity envisaged there.

Nevertheless, some of the special characteristics of systems engineering, as they were described in my preceding discussion, come out clearly in the excerpt. It is particularly important to note that the design process starts with a close scrutiny of the needs of the final users—the operating telephone companies who would provide service to the public. Essentially the technological process proper started by matching these needs with the available technology as represented by the research and development men in the organization. Moreover, the technological process ends only with a final determination that the apparatus will be satisfactory to the users, or, as the author says, “worthy of introduction into the Bell System.”

Systems engineering was given its explicit charter in Bell Telephone Laboratories in reorganizations that took place soon after World War II. The newly defined systems-engineering charter was described by Dr. M. J. Kelly, President of Bell Telephone Laboratories, in a lecture delivered to the Royal Society in March 1950. After describing the functions of the research and development organizations, Dr. Kelly proceeds as follows:

One of the principal responsibilities of systems engineering is technical planning and control. In the planning an appraisal is made of the various technical paths that can be followed in employing the new knowledge obtained by research and fundamental development in the specific development and design of new systems and facilities. The determination of the most effective use of new knowledge in the interest of the telephone user is the guiding principle of the planning studies. The most effective use may be the creation of new services, the improvement of the quality of existing services, the lowering of their cost, or some combination of these three . . .

Systems engineering also maintains close association with our research and fundamental development work . . .

It integrates the knowledge from operations, research and fundamental development, and specific systems and facilities development. With this as a background, it makes exhaustive studies that appraise and programme development projects for new systems and facilities. Each study outlines the broad technical plan for a development, its objectives, and its economic service worth. In many of the studies it is recommended that no development be undertaken at the time . . .

. . . Service trials are generally needed during the course of development. It organizes the trials in cooperation with operating engineers and participants in the tests and the evaluation of results . . .

Systems engineering has another important responsibility. It recommends the levels of the various technical standards that are important elements in determining the quality and reliability of telephone service . . .

Since Dr. Kelly made these statements there has perhaps been a tendency to place less emphasis on functions of internal technical control and more on needs and opportunities of the external world. In essentials, however, the charter sketched above is still the one by which our systems engineering operates. The remaining differences are only those that re-

sult naturally from nearly 20 years of technological evolution. For example, the rapid growth of technology since the war has generally meant that there are more physical ways in which a given function might conceivably be performed. Thus, the systems engineer has more to choose from and should, if he is conscientious, spend more time in studying possible choices. This situation is compounded by the fact that contemporary systems are generally more ambitious than their predecessors, and thus require more study on this account also.

When there is a large number of possible paths, economic considerations are likely to be paramount. Thus, our contemporary emphasis on finding minimum-cost solutions is perhaps greater than appears in Dr. Kelly's account. Because of the wide variety of situations that may present themselves, it is generally necessary to reduce all economic considerations to an "annual cost" basis in which capital costs, depreciation and obsolescence, repairs and maintenance, and so on, are combined in a single number.

Another problem that confronts the systems engineer increasingly is that of making new systems that are compatible with existing systems. In the telephone network, of course, everything must work with everything else, and some of the apparatus may be 40 years old. There is no possibility of a fresh start. The problem of designing new systems that can be introduced into the existing network without disruption has always been an important one for the Bell System. With the increasing diversity of apparatus in service, and with the radical changes implied in, for example, displacement of electromagnetic apparatus by purely electronic devices, the problem becomes more and more of a challenge to the wits of the telephone systems engineer.

Finally, the rapid rate of technological advance generally, and the desirability of taking technological steps worth making inevitably introduce some elements of technological risk-taking. In addition to some technological risks themselves, one must take risks in the commitment of substantial funds as well as scarce and valuable manpower throughout the technological process. An increasingly important responsibility of systems engineering is the elucidation of the nature and magnitude of the risks involved in such situations.

### Examples

This section will include several examples of systems engineering studies in the past by the Bell Telephone Laboratories. The examples are in some ways rather misleading, since they are all concerned with relatively large systems. The daily occupation of the systems engineer, of course, includes small problems as well as large ones. However, it is difficult to convey the motivation behind the smaller and more specialized studies adequately. The examples may also be misleading because they

are all concerned with systems requiring substantial technological advance as well as systems engineering in the narrow sense. This choice was made deliberately, however, since, as noted earlier, these should be the most important as well as the most difficult situations for the future.

Since the studies were so large the systems engineering work cannot be described in detail. The reader should pay particular attention to the interrelation between systems concepts and physical advances, the size of the technological advance undertaken in a single step, the existence of competing systems concepts, the influence of scale effects, and, finally, the approximate time required for a complete systems development.

### *Microwave Relay Systems*

The first example is furnished by microwave radio relay systems, now used extensively for the long-distance transmission of voice and television messages. It was, of course, recognized early in the history of the Bell System that radio, as opposed to wire, transmission furnished one possible means of providing commercial communications. This possibility was in fact exploited to some extent in the years before 1930, using high-frequency (HF) radio signals to overcome especially difficult geographical barriers such as oceans. However, the further exploitation of the radio spectrum in this range was very unpromising. The available frequency spectrum was very small, and had to be conserved for vitally important services; the level of man-made interference was high, the geographically separate communication links still tended to interfere with one another electromagnetically unless they operated on different frequencies, so that the frequency spectrum could not be readily reused, and so on. In the mid-1930's, tentative efforts were made to exploit the very-high-frequency (VHF) region instead, but the improvements that seemed in prospect from such a change were not very exciting.

Near the end of the 1930's, it was suddenly realized that most of the difficulties with early systems could be overcome if we took not a small but a large step in the frequency spectrum. A step of the order of at least 10 or 20 to 1 from the VHF band, to put us squarely in the middle of the microwave range, seemed to be called for. The level of man-made interference here was small, and available spectrum space was adequate. Perhaps most important, at these wavelengths highly directive antennas of moderate size were conceivable, so that non-interfering transmission links using the same wavelength could be constructed.

As a result of these possibilities, some tentative proposals of systems involving relays every 30 or 40 miles were advanced by the Bell research department about 1940–1941, and were given preliminary systems-engineering attention. Short-distance systems embodying the idea were provided the Signal Corps for field use during World War II, and the lead

was picked up for commercial use as soon as war pressures permitted. An experimental system was completed in 1947 and the first commercial system appeared a few years later.

In this history one should note the following: Although work on microwave circuitry had gone on all during the 1930's, and the key invention of the klystron had been made, much of the detailed technology of microwave systems was still lacking when the concept of the microwave relay system was first advanced, and we also had too little knowledge of propagation characteristics in the microwave range. Thus, the first result of a systems-engineering examination of the problem was to pinpoint and give direction to further exploratory work needed to supply the missing knowledge. Much of the work of field trials and field tests generally was directed to these ends. It turned out that the propagation characteristics of low-angle microwaves, in particular a propensity to deep multipath fading, was the most critical technical question. It had to be overcome by sophisticated circuitry and the introduction of relatively new modulation schemes to make the system successful.

Moreover, it is noticeable that the system was highly scale-sensitive. The change from normal radio frequencies to a much higher part of the spectrum was, of course, itself a kind of scale change. It also turned out, however, that the microwave system itself was efficient only for rather broad transmission channels. Moreover, since a large part of the cost of the final system went to the provision of towers, access roads, power supplies to the repeaters, and so on, it paid to provide numerous separate radio links at each site. Thus direct economics also argued in favor of big systems. The system was in fact well matched to the demand for broad frequency bands, which developed at the same time through the advent of commercial television. The clear prospect of such a demand was, of course, the primary justification for undertaking the development.

Finally, it is worth noting that the microwave relay system had a competitor in the so-called coaxial-cable system, on which work was started in the years just before World War II. Here the problems of manmade interference were overcome by a high degree of cable shielding. The coaxial system was also developed and went into commercial use, a few years ahead of the microwave system. For some years, it seemed rather more expensive. However, the principal technical problem associated with the coaxial system is the need for a large number of intermediate repeaters to overcome the high line losses. In recent years, the advent of the transistor, with its low power-supply requirements, permitting the easy introduction of intermediate repeaters powered directly through the line, seems to have reversed this economic advantage. Thus, one sees again the controlling importance of physical and technological advances in systems planning.

## **NIKE-AJAX**

The second example is the NIKE-AJAX missile. This is a surface-to-air antiaircraft missile on which design studies began in 1945 and which went into operation about 1953.

During World War II, the principal defenses against bombing planes were, of course, interceptor planes and ground-based artillery. Interceptors were fairly effective, but ground-based artillery was relatively ineffective, at least for reasonably high altitudes, because the target airplanes could too easily avoid the shell by a slight change in course while the shell was en route from the gun. Obviously, a missile was needed that could be guided all the way to the target to counter such evasive maneuvers.

The NIKE-AJAX system represented an effort to provide such a solution, capitalizing on relatively new advances in rocket propulsion and electronics. The system, aside from the missile proper, consisted essentially of two radars, one tracking the target airplane and the other the missile. The outputs of these two radars were fed to a computer that used the information to give a continuously updated estimate of the velocities of the missile and target and the course changes required of the missile to provide an interception. The required course changes were then transmitted to the missile by a data link. This is called a command-guidance system.

The system provided almost endless opportunities for the systems-engineering group to make trade-offs studies among such quantities as missile size, warhead size, amount of propellant, flight time versus trajectory shape, terminal maneuverability versus aerodynamic drag, and so on. These are too complicated to examine here. The most critical systems decision turned out to be the adoption of the command-guidance principle to begin with. There were, of course, other possible guidance principles that might have been adopted. A homing system, using homing apparatus in the missile operating on a radar signal reflected from the target, was perhaps the most attractive.

To the layman the merit of a homing system seems obvious. One gets better and better information as he approaches the target more and more closely. In fact, however, the situation is more complicated than this. A homing head provides really accurate information only in the immediate vicinity of the target, and the missile may or may not be able to make an adequate course correction in the time available before it goes past. Thus, homing is a better principle in a tail-chase, where relative velocities are low, than it is in a nearly head-on encounter, which would be more nearly typical of most antiaircraft engagements. Homing is also relatively better at long ranges, where the radars in the command-guidance system cannot be expected to give accurate information, than

it is in close. There are also subsidiary considerations involving the cost of reusable equipment on the ground as compared with simpler equipment in each expandable missile, problems of maintenance and reliability, and so on, which enter into any complete comparison of the two systems.

As it turned out, the choice between the two systems depended in an indirect way upon the question of range. The rocket propulsion readily available at that time did not guarantee very long ranges at best. The problem was particularly serious for a homing system, where typical trajectories lay mostly in dense air, involving high aerodynamic drags. The command-guidance system was superior in these circumstances because it could allow great freedom in choosing aerodynamically efficient trajectories and still lead to an interception. Thus, it could make the most of what propulsion there was. The question of accuracy at extreme ranges was vacuous as long as the ranges were not attainable in any event.

Thus it was determined that we would use the command system, taking advantage of it to eke out all possible range for the missile, and depend upon very sophisticated design in the ground-based radars and computers to provide adequate accuracy for any range we might be able to realize. The alternative path of trying for a substantial improvement in propulsion directly was forewarned as being incompatible with the essential strengths of a primarily electronic organization. In the event, this turned out to be a very fortunate decision. Radar and computer improvements were more than had been counted on, and the system, or its big brother, NIKE-HERCULES, was still effective when propulsion improvements increased the attainable range by several times.

### *Telstar*

The third example is the Telstar satellite. This will be remembered as the communications satellite put up by the Bell System about five years ago. Strictly speaking, it is not an example of finished systems engineering, since it was intended only as an experimental vehicle. However, the experiment accompanied some rather intensive systems-engineering studies in the same field, and perhaps can be introduced here on that basis.

An additional reason for introducing Telstar is the fact that it exemplifies particularly well one of the important aspects of systems engineering, especially in complex new situations. This is the necessity of making sure that one has an adequate and broad enough base of new science and technology before proceeding. Telstar, and other recent communication satellites, are in fact distinguished by the way in which they draw on technology in many directions. The dependence of any of these satellite systems upon the development of launching rockets is, of course, obvious.

However, many other elements also were needed. For example, communications satellites like Telstar are frequently described as microwave repeaters in the sky. They operate in the same frequency band as the microwave relay systems described a little earlier, and draw on this whole technique extensively. In the Telstar experiment, a low-noise ground receiver was also important. Thus, the invention of the maser, if not quite an indispensable step, was a very important one. So also was the introduction of the high-precision antenna and tracking system used in the experiment. Even the invention of the solar cell as a means of supplying prime power to the vehicle can be properly taken as a critical step. One has only to see the amount of effort that has gone into power-supply work for other kinds of space vehicles to recognize how important a problem this can be. Thus, the basic lesson to be learned here is that sometimes the best policy for a systems engineer is simply to wait until he has accumulated enough pieces of his jigsaw puzzle to make success reasonably likely. This, of course, does not deny the occasional desirability of going ahead on a modest and tentative basis, as in the microwave relay case, before the physical facts are all in. It does say that there is no real substitute for excellent technical judgment in knowing when to make full-scale commitments and when to wait.

Telstar is also interesting because it illustrates again the existence of competitive systems concepts. Telstar itself was intended as a prototype for a medium-altitude random-orbit satellite system. As an experimental device it was intended primarily to test out the ground-based receiver and tracking systems that such a system would require and the effects of the Van Allen radiation belts on vehicle electronics at these altitudes.

The competing concepts is, of course, the high-altitude synchronous satellite, which appears to have taken the lead at the present time. The high-altitude satellite avoids the problems of a complicated ground tracking system and it can provide continuous service with a very small number of vehicles. On the other hand, the propulsion requirements to put it in place are much greater (from most launch sites it takes more propulsion to get into a stationary orbit than it does to make a hard landing on the moon), a more elaborate antenna system is required to overcome the greater distance to the ground, and there are worrisome problems in connection with propagation delay, particularly for two-way conversations. A scale effect of a sort is also evident when one studies the economics of the problem. A random-orbit medium-altitude system tends to show up best with large amounts of traffic well distributed around the globe and is less well adapted to other situations.

### General Remarks

The examples just discussed were chosen primarily to illustrate some special aspects of systems engineering that seem to deserve particular

stress in any thorough discussion of it. The intended points are the following:

The first is the fact that in all cases the new system involved the exploitation of some definite advances in physics or electronic technology. I believe this is likely to be important whenever a substantial advance is involved. Systems engineering as an isolated discipline, in other words, should not be expected to provide enough advantage in most situations. It needs new technology to work with. The proper understanding of the implications of technological advances and their effective utilization, on the other hand, are also difficult problems and are, of course, prime responsibilities of the systems engineer. Thus, systems engineering and the basic technology must be coordinated if maximum progress is to be made.

In the second place, in the microwave-relay and NIKE-AJAX examples, at least, preliminary systems planning took place while essential physical knowledge and some needed technology were both still lacking. Thus, one of the important early contributions of systems analysis was the direction of applied research and exploratory development studies to areas of critical importance before a final systems concept could be generated.

In at least the NIKE-AJAX case, the final systems concept involved the tacit assumption that state-of-the-art advances in some critical areas would be made during the course of the development. It is important to note, however, how carefully these areas were chosen. They were, in fact, critical to the system, and they were in fields in which Bell Laboratories was particularly at home. State-of-the-art advances in other directions, however welcome, were not indispensable. The systems engineer who asks for a better result in every direction than the world has yet provided is simply not practicing his trade.

In the third place, scale effects, particularly if the term is broadened to include such considerations as absolute position in the frequency spectrum, turned out to be of controlling importance in several cases. I believe this is still more likely to be true for many of the very broad questions involving environmental control, transportation, and the like, which should be of particular interest to the House Committee. Thus, systems concepts that are appropriate and valuable in one set of circumstances may have no meaning in different situations. It is the business of the systems engineer to understand such questions well, and to have clearly in mind how the scale of the problem may affect the appropriate technology.

Finally, it is important to notice that in each of the examples there was at least one competing systems concept in addition to the concept directly under discussion. The existence of such competing systems can be of enormous value, particularly if they can be studied objectively by



the same systems-engineering group. The result is likely to be much better insight into the problem than could be obtained by working with one systems concept alone.

On occasion, of course, competing concepts may lead to the emergence of a third concept, better than either. If the new concept is really valuable, however, it is likely to have its own distinctive character, and not be merely a fusion of the others. Thus, to go back to the earlier analogy, a house can be Georgian, modern, or colonial, but it should not be a little of each. The fox and the rabbit are each biologically successful animals, but a compromise animal would have no natural ecological niche.

The set of examples also brings out one more point that may not be quite so apparent. In each case, the basic systems analysis was done by men who had already spent several years in the same or related fields. They were thus in a sense professionals, qualified through subject matter knowledge as well as systems understanding.<sup>1</sup>

I believe that this is the inevitable pattern for any effective attacks on our well-known problems—transportation, environmental control, and so on. At first, the visiting systems engineer with no substantive knowledge of the field, or the visiting scientist engaged in a summer study, may be able to make a contribution. As the field develops to the point where sustained intellectual hard work is necessary, however, the men who have developed a sort of professionalism because they work in the area continuously will be increasingly the men who count. They may or may not call themselves “systems engineers,” but they are in any case the men who understand the field in enough depth to give it structure and continuity.

In the long run, then, the country's future on these problems will depend on finding ways of developing such professionalism. The suggestion made in one of the other papers, proposing quasi-permanent research institutes, is one way of solving the problem. As such it deserves careful attention. On the other hand, the chartering and leadership of such institutes over a period of time is a most difficult problem, and needs to be approached with great care. My point here is that this sort of step, and not pick-up summer institutes or quick systems-engineering studies, is a requirement for serious attempts to address ourselves to the country's largest problems.

### References

1. *Methods of Operations Research*, p. 10a.
2. Morse and Kimball, p. 2.

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<sup>1</sup> See, for example, the question of “enduring themes” in the report by J. B. Fisk to the Daddario Committee on December 11, 1963.

## APPLICATION OF BEHAVIORAL SCIENCE

by RAYMOND A. BAUER

It can be taken as axiomatic that any problem that involves human beings or their interests may at some point benefit from the application of systematic knowledge about man and his institutions. The acquisition of such systematic knowledge and the development of methods for the orderly study of man's behavior and institutions have been the business of that group of disciplines known as the behavioral sciences. The behavioral sciences are usually understood to include anthropology, psychology, and sociology as the three major disciplines. Since human behavior is biologically based, the behavioral sciences includes also parts of zoology, genetics, anatomy, physiology, and so on. On the social end of the continuum, some of the work in the following disciplines is also relevant: political science, history, demography, statistics, and economics.

In varying degrees the methods or findings of these disciplines have been applied to human affairs for decades. The use of tests for measuring ability is probably the most widespread and familiar such application. In addition, there is the application of psychological and sociological techniques and concepts in marketing research, new methods of instruction via programmed learning, and new concepts of the learning process, revision of methods of management, treatment of mental illness, an understanding of the social components of physical illness and its treatment, the application of communication theory in advertising, the gathering and analysis of social data for planning, even the use of "voice prints" for identifying persons.

The list of established applications of behavioral science findings and methods is long, and has been recited often.<sup>1</sup> Another assessment would add little to what has been said many times before. It seems more useful to discuss the general problems of the use of behavioral science services,

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<sup>1</sup> A recent survey of the relationship of the behavioral sciences to practical problems and the consequent need for the further development of the behavioral sciences is: *An Overview of the Behavioral Sciences*, a conference report submitted to the National Institutes of Health, Fall 1966. A summary of the substantive findings of the behavioral sciences is offered in Bernard Berelson and Gary Steiner, *Human Behavior: An Inventory of Scientific Findings*. This is a useful summary despite some reservations I will enter later.

findings, and methods, and—in this context—to present some special opportunities that seem imminent in the area of policy planning and control.

The topics to be discussed are, in order:

I. General Considerations:

- A. Problems in the Application of Behavioral Sciences.
- B. The Nature of Behavioral Science Concepts and Findings.
- C. Behavioral Science Methods.

II. New Tools for Planning and Control:

- A. Study of Policy Formation.
- B. Social Indicators.

Various readers will have differing interests in the two sections of this paper. The more general and abstract considerations of the nature and usefulness of behavioral science findings, concepts, and methods are in Section I, and the substantive promise of the two areas of application in Section II. Each individual reader ought to feel free to use this document to suit his own interest. Either section can be read first, and each can be read independently of the other.

## 1. General Considerations

### *A. Problems in the Application of Behavioral Sciences*

While it is true that the behavioral sciences are potentially relevant to any problem that involves people, it cannot always be contended that the behavioral sciences should be the preferred source from which to seek a solution. Nor can it be contended that if the behavioral sciences *are* the preferred source, the prospects for usefulness are necessarily high. In any given instance, the behavioral sciences are in competition with both present practice and with the natural sciences, and subject to their own inherent limitations regardless of the status of present practice or the capability of the natural sciences.

1. Professor Harvey Brooks points out in his paper, "Applied Research, Definitions, Concepts, Themes," that at any point in time a given human problem may or may not be "society limited," i.e., the limiting factor is human beings or their institutions. Even a problem that appears at a given point in time to be "society bound," such as population control, may be susceptible to what Weinberg calls a "technological fix." In the instance of population control, as Brooks says, "the introduction of IUD's (intrauterine devices) has sufficiently lowered the threshold of persuasion for family limitation to considerably alter the outlook for population control." Hence, whether a behavioral or natural science solution should be pursued is an empirical question to be raised in each separate instance.

Furthermore, a problem that is "society limited" may not be one that behavioral scientists are at the time equipped to solve. Some aspects of

the treatment of mental illness are limited by inadequacy of systematic knowledge (which is not to say that progress cannot be made). Sometimes methods are inadequate, as in the case of our inability to isolate the long-term effects of mass communications.

2. Perhaps more important than limitations on systematic behavioral science knowledge and methods is the circumstance that most behavioral scientists, just as most natural scientists, have little contact with, understanding of, or sympathy for action problems. I will deal with this issue more fully when treating of new approaches to the study of policy formation. It is, however, an issue that affects a wide range of areas of potential application. While behavioral scientists are accustomed to doing applied work of a more limited technical nature—assessing personnel, specifying the human requirements for machine design, discovering consumer attitudes, and so on—they are less acquainted with and less sympathetic with the sorts of information that a manager, statesman, or any other practitioner requires to handle his job *as he sees it*.

Very good work has been done on *policy-relevant* research, that is, on gathering data or understanding phenomena with which the policy maker must deal.<sup>1</sup> Such data and understandings are valuable inputs into the policy process. The contribution of such policy-relevant information is much more familiar than the study of the process in which it is used.

When the better behavioral scientists have studied matters that relate to complex action problems, they have often done one of the following things: used the action arena as a convenient place to study systematic problems that had no bearing on the applied problem (e.g., studying a supermarket buying committee as an example of group processes independently of how this process affected the outcome); taken some dimensions of the problem that they are accustomed to studying and tried to reduce the overall problem to those dimensions (e.g., seeing war as a displacement of aggression); proposed the adoption of a general formula that might apply to a class of problems, and might be useful if one did not know the specific situation (e.g., a model of "good leadership" in an inappropriate context). Obviously, favorable examples can be cited. Here, however, I am recounting only the nature of the difficulties so that the limited success of the past can better be understood, and so that future expectations will be realistic.

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<sup>1</sup> An especially good description of such policy-relevant research can be found in Likert, Rensis, "A Statement Submitted to the Research and Technical Programs Subcommittee of the Committee on Government Operations, House of Representatives, in response to questions raised in an 'Inquiry on Federally-Sponsored Social Research,' December 2, 1966." Professor Likert is Director of the Institute for Social Research, The University of Michigan. The Institute of Social Research has conducted a number of programs on policy-relevant questions since World War II, and made very distinguished contributions.

3. A broad limitation on application of behavioral science knowledge stems from the people and institutions who might benefit from it. I will not labor the fact that some practitioners resist help when help is, in fact, available. Less understood, however, is the difficulty of using such help in an organization that is not set up to use it. I will take my example from an area of application—namely, marketing research—in which it is quite well accepted that people know how to use behavioral science knowledge and methods.

The most direct problem posed by the use of marketing research information is that such information is seldom neutral. It favors one course of action over another, proves one man right and another wrong. This dampens the enthusiasm of some persons for any information whatsoever, and sharpens the ingenuity of others in interpreting data to their advantage. A further difficulty is that the procedures for the routine conduct of business often are not always sufficiently orderly that research can be commissioned in time to be useful. Methods of rewarding personnel may make it advantageous to act contrary to the findings of research. And so on. Difficulties can be minimized, but only if the firm has a far better than average decisions-process structure and more than average discipline in adhering to decisions. Business consultants can testify that difficulties of this sort are routine, and very resistant to correction.

The creation of the proper organizational environment for the use of applied behavioral science can be one of the most difficult behavioral science problems of all.

4. As would be expected from the experience of the natural sciences, the status of applied behavioral science is second to that of basic behavioral science, creating the familiar problem that the first-rate people are more likely to go into basic research. Furthermore, money for the development of applied research, except in the field of mental health, is often more difficult to come by than is money for basic research. For example, fundamental research relevant to marketing is generally too applied to attract foundation or Government money, and has too little *immediate* payoff to get business sponsorship.

While the behavioral sciences share all the problems that are cited in the application of the natural sciences, some of them are especially acute.

The relative scarcity of funding for application of the behavioral sciences, mentioned above, is especially marked. There is a considerable demand for behavioral scientists who will work on immediate operating problems and supply immediately actionable information. The result of this demand is to suck relatively mediocre behavioral scientists into business, in particular, where they can maintain outside contacts only at great effort. It is extremely rare to find a sizable group of behavioral scientists

who have the dual orientation toward "inside" action problems and "outside" scientific problems, which Brooks (this volume) indicates is so desirable. I suspect that this difficulty lies primarily in the lack of any real confidence among financing agencies that the behavioral sciences can contribute anything beyond "useful data." There is no firmly established confidence that basic findings have relevance for practical applications, and perhaps even less is there understanding of the qualities of mind and the kinds of activities required to make basic findings applicable.

5. The applications of behavioral sciences that are accepted with least argument tend in general to involve the use of behavioral science technology to gather factual information, the necessity for which was agreed upon in advance. The gathering of data on the attitudes of World War II troops as aspects of demobilization policy might be such an example. By and large, the use of standardized tests to measure ability and achievement—with some side quibbles—is another example. Tests of the relative effectiveness of communications on different population groups would probably be another example. Most reasonable persons will grant readily that behavioral scientists have developed the technology for answering such questions, and that in general the answers are useful.

Contributions from behavioral science concepts and theories are more subject to controversy. Here we might cite, as a *good* example, the use of tests of personality for the purpose of assessing an individual's ability to make full use of his capacities as measured on tests of ability. Or we might take the more elaborate theoretical structure offered by Professor David McClellan to relate child-socialization practices to the development of adult patterns of motivation, and thence to the societies' capacities for economic development.

I will have more to say about behavioral science concepts in the next section of my paper. At this point, however, I wish to assert that the fact that it is easier to gain assent to contributions *via* behavioral science technology than to those *via* behavioral science concepts must be regarded at least in part as a function of the fact that the technical contributions leave far less room for argument. One must guess that the more profound contributions are, and will be, on the conceptual level. For example, while there is still strong controversy over many aspects of Freudian personality theory, most of its fundamental features—repression, unconscious, and so on—have worked their way into the common language. Our whole way of thinking about human behavior has been influenced.

### *Problems Summarized*

This itemization of problems in the application of the behavioral sciences is made in part to indicate my own awareness of the difficulties in making such applications. But it is also intended as an exposition of the *nature* of these difficulties so that they may be recognized, understood, and corrected.

Responsible judgment must be made as to whether or not a particular problem at a particular time will in fact benefit more from the application of behavioral sciences methods or knowledge, or from a technological approach, or from the continuation of present practice as developed by experienced practitioners. Behavioral scientists will have to become more familiar with and sympathetic toward action problems. And, finally, the beneficiaries of behavioral science methods and findings will have to prepare themselves better to use them.

### ***B. The Nature of Behavioral Science Concepts and Findings***

The behavioral sciences have been ridiculed on the one hand for documenting the obvious and on the other for asserting the implausible. Furthermore, empirical findings in some areas of inquiry have been stated in such a form that they either have the appearance of contradicting each other, or they represent such a proliferation of possible relationships that it is often complained that all a behavioral scientist can assert is "It depends." If all the above assertions were justified, it would still be possible that the methods of behavioral scientists would be useful for empirical research on applied problems. However, it would bode ill for the notion that the behavioral sciences have any systematic body of knowledge to bring to bear on those problems.

The broad criticisms of social science findings and concepts stem from a lack of understanding of behavioral science propositions, which behavioral scientists themselves have done little to correct. Even more generally, it would appear that they have done very little thinking about the nature of these propositions; thus they are ill equipped to answer the charges. In 1964, two very prominent behavioral scientists, Bernard Berelson and the late Gary Steiner published a book entitled, *Human Behavior: An Inventory of Scientific Findings*. The distinctions that I shall make here are not generally made by them, with the result that they left themselves open to at least one scathing review in which they were attacked for presenting the obvious in a pontifical style.

The following comments are intended to throw light on behavioral science propositions from the point of view of common misunderstandings.

1. Certain propositions ought to be taken as axioms rather than as statements that might evoke a modicum of surprise, or that would warrant expenditure of research effort to substantiate them. Berelson and Steiner cite one such proposition: "The larger the organization becomes, the more ranks of personnel there will tend to be within it." As stated, it is quite difficult to imagine this proposition not being true. Yet, as an axiom, it is a useful reminder, and a point of departure in the investigation of less obvious propositions.

2. Ability to produce an intended effect by experimental manipulation is not a trivial accomplishment in any discipline. A certain amount

of experimental behavioral science should be evaluated as an exercise in experimental manipulation rather than as discovery of novel propositions. For decades, wags defined a sociologist as a man who would spend \$20,000 (in 1936 dollars) to find a bawdy house. No one had the wit to rejoin that as a methodological exercise it might well be worth the money in question to have found a new way to such a locale of pleasure. Unfortunately, one cannot be certain at all times that researchers are clear in their own minds as to whether they are testing a proposition that is not self-evident, or testing their ability to produce certain experimental effects.

3. Many apparently self-evident propositions are not self-evident. In fact, with a little bit of diligence one can ordinarily find a proposition equal in plausibility to another proposition, but completely contradictory of it. For example, it is taken for granted that children will rebel against their parents at some point. Yet, this is a proposition opposite to the self-evident notion that children learn their values at home!

Many "self-evident" propositions simply are not true. For example, one would certainly expect that soldiers in units with high rates of promotion would think that their chances for promotion were better than those of soldiers in units in which advancement was not as frequent. World War II research found that this was not so. Soldiers in the Military Police, a group with very slow promotions, felt more sanguine about their chances than did troops in the Air Force. It would appear that soldiers judged their chances relative to those persons immediately around them rather than according to some standard embracing the entire Armed Forces.

In other instances, apparently contradictory and equally self-evident propositions may both be true, *under some specifiable conditions*. The trick here is that most meaningful behavioral science propositions are contingent propositions. Unfortunately, here too behavioral scientists have not been either sufficiently conscious of this condition or sufficiently communicative about it. Both the propositions I cited above are in some sense true. Children do learn their values at home. But, there are times and circumstances when children also rebel, and some children rebel to the extent that they end up acting quite contrary to their parents values—e.g., the traditional minister's son in American society. It is possible to make a reasonable generalization as to the circumstances under which such rebellion will take place, such as when the parents are being overly committed to their own values and overly insistent on their children adopting them. The boundaries between reasonable and "over" commitment on the part of parents must be established empirically. We can, however, make the non-self-evident prediction that at *some* point the relationship between the values of children and their parents commitment to their own values will reverse itself.



4. I said that most meaningful behavioral science propositions are contingent ones. This assumes that we are talking about propositions of an intermediate level of abstraction. Certain empirical propositions are simple descriptive statements of a low level of abstraction; e.g., American adults on the average show a low level of interest in international affairs. In that form ("on the average") the proposition is true and noncontingent.

On the other hand, some propositions on a high level of abstraction are also noncontingent. For example, informal organizations tend to become more formal, particularly as the organizations get larger. Such propositions are of such a high degree of generalization that they can coexist with equally true, uncontingent, contrary propositions, such as that informal organizations tend to develop within the framework of large formal organizations. As with other disciplines, the interesting work of the behavioral sciences begins when one can specify which of the tendencies that exist in the abstract will prevail. In many areas of the behavioral sciences this is precisely the sort of development that is taking place. Apparently contradictory findings are actually contingent findings that state that one pattern will obtain under some conditions and another pattern under others.

#### *The Contingent Nature of Propositions Illustrated*

I should like to describe a single experiment to illustrate both how a self-evident proposition can be challenged, and how a series of contingent findings could be predicted on the basis of existing systematic knowledge.

The experiment had to do with a very old problem in Western culture—the extent to which the credibility of the person delivering a message affects the extent to which the message will be believed. Since Aristotle's *Rhetoric* there has been for practical purposes no challenge to the notion that the more credible (powerful, honest, knowledgeable, likeable, what have you) a communicator is, the more effective his message will be. Yet there has been evidence in the research literature that *perhaps* a communicator could be *too* credible, in the sense that persons listening to or reading his message might accept his conclusions too readily and not examine the evidence. There was also evidence that after a few weeks people forget where they heard or read messages, i.e., the "source" lost its effect.

The experiment to which I will refer, conducted by Murray Hilibrand,<sup>1</sup> was designed to test the proposition that a person exposed to a message from a person who was seen as highly competent and highly trust-

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<sup>1</sup> Hilibrand, Murray, *Source Credibility and the Persuasive Process*, unpublished doctoral dissertation, Harvard Business School, 1964.

worthy would be very vulnerable to a counter message after the passage of a few weeks time. This prediction was based on the assumption that the person receiving the message would not examine the evidence critically, but would accept the conclusions in an unthinking fashion. It was also assumed that the passage of some time would be required for the person to forget the source of the communication. The following results were obtained as predicted: (1) A person perceived as highly competent and highly trustworthy proved to be more persuasive initially than anyone of three other types of persons; highly competent but not trustworthy; not competent but trustworthy; neither competent nor trustworthy. The last of these sources produced the worst results. (This is completely consistent with a long tradition of research and common sense.) (2) The "best" source, highly competent and highly trustworthy, proved most effective when people were exposed to a counter message shortly after the initial message, i.e., before they had a chance to forget the source. (3) When a two-week time period had elapsed, those persons who had read a message from a highly competent, highly trustworthy source were *most* vulnerable of all to the counter message. As a matter of fact their vulnerability was so great in the instance observed that people exposed to this source ended up with a net position worse than those persons who had read a message from a source they perceived as low in both competence and trust. (4) Not predicted precisely, but fitting the pattern, was the finding that the most effective source was one who was seen as *highly competent but not very trustworthy*. It would seem as though reading a message written by a person like this puts one in a frame of mind that might be verbalized as follows: "This guy knows what he is talking about, but I'd better watch out for what he's saying." The result would appear to be that persons in such a frame of mind scrutinize the evidence carefully enough so that they are inoculated to counter arguments. That is to say that in proceeding with caution they have probably considered the counter arguments while still having access to the original positive arguments.

The same experiment was carried out with the counter message being delivered a few minutes, rather than a few weeks, after the original message. Without the longer intervening time period in which they could forget the source (from which they had heard) of the original message, persons exposed to the "best" source were not vulnerable in the fashion mentioned above.

By now, I would assume that all these propositions are also self-evident, except for the fact that they had not been evident before the experiment was executed.

Note also the extent to which the results were contingent on the interaction of these variables: perception of the source on two dimensions, competence and trust; time after exposure to the initial message; the

initial message; and the counter message. The effect of the initial message was contingent only on the perception of the source. The effect of the counter message was contingent on both the perception of the source of the original message, and the time elapsed after exposure to the initial communication. The extent to which people were favorable to the topic, Civil Defense, which was used as the subject matter of the message in this experiment, after exposure to the counter message, was dependent where he fell in the eight experimental cells representing the various combinations of times and sources.

### *The Nature of Concepts and Findings Summarized*

This brief essay on the nature of behavioral science concepts and findings touches only on those aspects of the topic that I thought might be useful answers to some of the more prevalent criticisms that one is likely to encounter and to which a reasonable man is likely to want an answer.

What I have said is essentially this:

If a behavioral scientist appears to be producing empirical support for a self-evident proposition, he may either be demonstrating his ability to measure a relationship about which one would have scarcely any doubt, or demonstrating his ability to produce a phenomenon under experimental conditions. Neither of these achievements is trivial. It is the unhappy truth, however, that researchers are not always clear in their own minds as to which of these enterprises they are involved in.

"Self-evident" propositions may not be so. They may be wrong. They may appear to be self-evident after the fact. (Many investigators have suffered under the realization that after the data were gathered, the inconceivable had become obvious.) And, equally "self-evident," but contradictory, propositions may both be true, under some circumstances.

The preceding statement implies what is probably the most important of the statements in my discussion, namely, that the important and interesting behavioral science propositions are contingent propositions. The impact of any phenomenon must be presumed to be dependent upon the situation in which it operates. Seemingly contradictory propositions must be assumed either to be findings that have not been stated in a properly contingent form, or to be in some way untrue. The possibility of findings or concepts being a result of ineptitude or of mistaken interpretation of the operation of chance should not be dismissed. But neither should we reject the possibility that the contingent nature of propositions may not have been properly explored.

All the above is in a way charitable to the behavioral scientist and to behavioral science. I have no desire in any way whatsoever to conceal,

disguise, or in any other way obscure the fact that behavioral scientists are not always certain as to what they are up to—testing propositions, testing their ability to measure, or testing their ability to replicate phenomena in an experimental situation—or that some behavioral scientists are obtuse. The issue should not be whether or not some or many or all behavioral scientists understand what they are up to. The effort here is to give the outsider somewhat better ability to evaluate what is going on, to make his own judgment when confronted by an argument presented by a partisan who nevertheless feels the need to confront the issues as they are seen by the opponents.

I would like to close this section with one unequivocally kind word for the point of view of the behavioral sciences. The more successful the behavioral sciences are in establishing their concepts and findings, the less credit these disciplines are likely to get as their point of view is absorbed into the common man's way of thinking. In past decades there has been a vast increase in the level of sophistication with which we all think about human behavior, whether it be mental illness, job motivation, the fact that our own words may reveal more than we intend, and so on. Certain notions once popular are now quite thoroughly demolished. The idea that intellectual ability is a solely inherited capacity that is race-linked has been so thoroughly refuted that it is virtually never raised seriously in the civil rights controversy anymore. In these respects, the behavioral scientists and all other intelligent men concerned with the systematic study of human behavior are indeed raising the level of "common sense" to the point where the distinctive contribution of the behavioral scientist can be established only through careful historical analysis.

### **C. Behavioral Science Methods**

The behavioral sciences use all the conventional methods of the natural sciences—observations, experimentation, and so on—but the technology of research in the behavioral sciences is generally much more modest. In addition, some methods are used that distinguish research in the behavioral disciplines from that in the natural sciences. Historians and archeologists, for example, consult records and traces of man's past behavior. In this procedure administrative records are sources of social statistics. More usual and distinctive, however, is the employment of interviews and questionnaires to gather data about people from the people's own words.

The development of the behavioral sciences, and of psychology in particular, has been marked by sharp controversy over methods, and over the use of "verbal report" in particular. The conflict in psychology was motivated in part by a desire to turn attention away from a preoccupation with man's subjective experience studied by introspection toward consideration of his overt behavior. Associated with this concern, however, was

a desire to mimic the methods of the established natural sciences. The period of the 1920's was marked by an enthusiasm for more objective methods of gathering and reporting data, and for experimentation as the preferred method of investigation. There can be little quarrel with the positive aspects of this development. Not equally salutary, however, was the confusion of some of the methods used by the natural sciences with the totality of the scientific endeavor.

1. The behavioral sciences are slowly freeing themselves of a certain amount of dogmatism about the proper methods of science and about the role of methods in science. To some extent the issue remains unresolved with respect to the use of interviews and questionnaires. In many quarters of the behavioral sciences, and among intelligent laymen, the answers people give in interviews or to questionnaires are regarded as second class data, less "objective" than observations of overt behavior, or instrumented readings of physiological states. Yet, what a person says can be detected and recorded as objectively as can the electrical conductivity of his skin. The statement, "Thirty-seven percent of a national sample of adults over the age of 18 said 'yes' to the question, 'Would you like to have more classical music programs on TV?'," is as objective in form and probably as accurate as that which can be made on the basis of any other sort of data.

2. The difficulty in the use of the answers people give to questions lies not in the nature of data themselves, but in the inferences that are made from them. If a person says he intends to buy an automobile, this circumstance can be observed and reported as objectively as any other event. The problem is: are we to conclude that when he says he will buy an automobile he really intends to buy an automobile, he is trying to make the interviewer happy, he is trying to make himself feel good, or, perhaps, he is part of a conspiracy to lead us to overestimate the rate of future car buying! The sophisticated behavioral scientist treats the responses he gets as *verbal behavior* that can be observed and recorded. However, as with other data, the behavioral scientist must then take the responsibility for the inferences he makes from the data. A good deal of experience has been accumulated concerning the makings of inferences. Furthermore, this experience has led to the practice of gathering a variety of verbal behavior on complex issues, so that the inferences to be made are based on a heterogeneous body of data. For example, attitudes on complex issues are seldom measured by a single question, but are inferred from responses to many questions. The analyst of such verbal behavior logically has the same problem as the interpreter of any other sort of data. As with other scientists, his interpretations can often be tested against further data, they can be tested for consistency with accepted facts, or they can be tested in the crucible of action.

The behavioral scientist is fortunate in that the object of his investigation can communicate with him. This circumstance makes possible the investigation of a wide range of problems with a speed and economy that could not be accomplished by other means. While it is true that there are conditions in which these communications can be misleading, and others under which they are crude approximations of what we are interested in, these limitations should not lead critics to discourage the use of this distinctive resource of behavioral science data.

3. Behavioral science research suffers, of course, from the varying competence of behavioral scientists in recognizing and communicating the limitations of the methods they use and the bases for the inferences they make from their data. Perhaps more important, however, is the prevalent underfinancing of behavioral science research. As a result, second-best methods are often used because of their economy. It is in this situation that the use of the interview as a substitute for other methods is to be most regretted. For example, people are regularly asked to report on events of a sort that it is known they can recall only imperfectly. These same events could be observed as they occur if the researcher could afford to have an observer or instrument, such as a camera or a tape recorder, on the spot. This situation is made more unfortunate by the fact that it breeds indifferent to quality. A study of behavioral scientists would probably reveal that a very high proportion of them have become so accustomed to improvising and compromising that they do so as a matter of habit even when and where it is not necessary. It would seem certain that given this condition, there is, in turn, a tendency not to pursue the development of newer methods that might prove expensive.

The underfinancing, the use of second-best methods, and a tacit defeatism do not by any means characterize all areas of behavioral sciences. Those areas of behavioral science that are closest to the biological and physical sciences are better financed and ordinarily use more sophisticated methods and better instrumentation. These areas include physiological psychology and psychoacoustics, for example.

The poverty pockets in behavioral science tend to concentrate on areas related to broad social problems, where large-scale data gathering is involved. Even students of social communication, with its presumed practical importance to such matters as advertising, have virtually never been able to conduct studies of the long-term effects of communications on people.

The sample-interview survey is the most usual device for studying public reaction to and experience with applied problems. A sample of about 2,000 persons is adequate to estimate with precision what proportion of people think the President is doing a good job, or favor one national policy or another. It is not an adequate sample, however, if one wants to study the factors that lie behind people's experience or feelings—for

example, which sort of people with which sort of background show a given pattern or reaction. This type of inquiry requires subdivision of the sample into smaller and smaller subsamples, a process that soon reduces the relevant data base to an intolerably small size. On rare occasions, samples of tens of thousands of persons have been taken. A study, *Equality of Educational Opportunity*, conducted in 1965, employed a sample of 900,000 students! This large a sample is extremely rare; however, it made it possible to isolate many of the factors in a highly complex situation and thereby to reach conclusions of reasonable firmness. Machine processing makes it possible to handle large volumes of data. Computers and computer programs make sophisticated analysis possible. Our model of how the events of the world are shaped makes complex analysis desirable. But we need more and better data to achieve the proper quality of work.

### *Methods Summarized*

Behavioral science methods in the hands of competent practitioners are generally adequate to *most of the systematic problems* with which they are presently concerned. Or, in any event, they are not likely to produce misleading results when their limitations are recognized. Improvement in the training of researchers and the development of better methods are, of course, desirable. Substantial advance in the use of better methods in the investigation of problems of social and economic importance, however, is hampered by lack of adequate financing, resulting in the use of second-best methods and gathering of inadequate data.

## II. New Tools for Planning and Control

Some measure of social planning and control has been accepted as necessary since the founding of the republic. Not even the most extreme partisan of *laissez faire* would argue against the desirability of using basic population data for estimating the forthcoming need for school rooms, or of using population data and income projections for estimating tax revenue. In the past two decades the acceptance of more planning and control—not as the antithesis to a free economy, but as the basis for preserving it—have come to be taken for granted. The success of economic indicators, and of the associated policies for stabilizing the economy and ensuring economic growth, have been to a large extent the basis for realization of the desirability of planning and control. In this section of my paper I will present two recent developments that promise to produce new instruments for planning and control.

1. In the past decade, behavioral scientists have arrived at what appears to be a useful approach to such complex phenomena as the formation of policy both in private institutions and in the public arena. There is no single event that marks this development. Indeed, many of the ele-

ments in the contemporary view of the policy process are not novel or may represent only more formal ways of stating familiar ideas. However, when this viewpoint is presented in a relatively systematic fashion it provides us with a more fruitful way of looking at policy formation. It frees us from some vitally misleading notions. From the point of view of the policy maker it promises three advantages: (1) more useful analysis of specific policy problems; (2) a framework within which the systematic work of the behavioral sciences becomes applicable; (3) an improved understanding of the policy-making process.

2. The usefulness of economic and social statistics has always been taken for granted. In the past two or three years, however, the existing statistical series have come under scrutiny, and both in the United States and in other countries a lively interest has developed in the possibility of more comprehensive societal information systems—systems of social indicators. This notion of a system of social indicators is only incidentally related to the more popular enthusiasm and concern over computer-based data banks for storing and retrieving available data. It is being proposed that we develop more adequate measures of those aspects of our society that are not covered by economic data, and more appropriate measures of social phenomena that we now measure poorly. The purpose is dual. For immediate purposes it is suggested that we need to be able to evaluate both our public and private programs in terms of a fuller range of criteria than their sheer economic impact, to be able to judge consequences in terms of the experiences and reactions of the people involved. Over the longer haul, it is expected that the existence of a wide range of data will enable behavioral scientists to develop a more adequate model of the noneconomic aspects of our society so that we may plan and control our affairs better.

These two lines of interest have relatively independent histories. However, taken together they offer us the following possibilities: (1) a better understanding of the processes whereby our most important decisions are made, and presumably an improved ability to make them; (2) a better model of the social system in which we are living, and thus a better basis for planning and controlling our actions; (3) better basic data for evaluating the consequences of our past actions and for guiding our future ones. These are convergent efforts directed at providing us with a better set of tools for planning and control.

These are the goals, not the attainments! The goals are attainable at various levels and with varying degrees of difficulty. For many purposes quite reasonable social statistics already exist, and our present statistical series are undergoing constant extension and revision. Our knowledge of the nature of our own society and our ability to plan and control our institutions is demonstrably adequate to achieve the present level of security and prosperity, and we are continually making advances in the art



of administration and governance. There are, however, specific reasons to believe that we may be at the threshold of significant new advances that, while they will not be entirely easy to make, warrant the effort because of the value of making even modest progress toward such goals as these.<sup>1</sup>

### A. *The Study of Policy Formation*

1. *The background of policy studies.* There is a class of important and complex events, which we call policy formation. It includes such occurrences as congressional decisions on Federal support of research and development, a business decision to enter the international market, a university decision to stress undergraduate instruction at the expense of its graduate school, a hospital decision to have an inpatient psychiatric service, an Executive decision to strengthen economic ties with Eastern Europe despite our engagement with Communist forces in Viet Nam. As contrasted to management of the day-to-day business of such institutions, these actions amount to the setting of new courses. They are the most important class of events in human affairs and are also the most complex and demanding with regard to social, intellectual, and moral problems.

The setting and implementation of policy have attracted much attention and comment over the centuries. It might therefore be assumed that the process must be quite thoroughly understood. But men have regularly exercised wisdom and skill when their knowledge was incomplete. Rocks were thrown, arrows fired, and cannon constructed and shot before ballistics were mastered. Bridges were built before the basic data on strength of materials and mechanics of stress were mastered. But, on the whole it has been mankind's experience that we are able to guide our affairs more to our liking as we understand better what we are doing.

Historically, policy studies have mainly been the business of political philosophers who suffered the occupational disability of continually (and without due warning) mixing up their ideas of how they wished the world were with their best guesses as to how it actually was. The most notable exception to this was a man who attempted to separate his preferences for what he thought should be from what actually occurred—Machiavelli. And we all know what this did to his reputation.

Students of business organizations, educational institutions, hospitals, and other nonprofit organizations have been almost as flagrant in their assumption of wisdom. Only a few have been sufficiently informed and

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<sup>1</sup> My concern with the potential of these areas has given me the opportunity to organize two task forces to explore their possibilities. The work of one task force, under the American Academy of Arts and Sciences, is reported in *Social Indicators*, (Raymond A. Bauer, ed.) M.I.T. Press, 1966. The work of the other, under the National Planning Association, is in press under the title, *The Study of Policy Formation*, (Raymond A. Bauer, and Kenneth Gergen, eds.) to be published by The Free Press.

self-critical to be aware of the flimsy base on which prescriptions for running such organizations have been posed. Considering the fact that most of our modern institutions have existed in substantially their present form for many decades, and in many instances centuries, it is sobering that only a few decades ago Chester Barnard, with long experience in business and great familiarity with the conventional wisdom, wrote:

Nothing of which I knew treated organization in a way which seemed to me to correspond either to my experience or to the understanding implicit in the conduct of those recognized to be adept in executive practice or in the leadership of organizations (1).

This state of affairs has been under radical change in recent decades. For approximately 40 years American political scientists have increasingly been insisting on a more naturalistic reporting of what has actually happened as policies have been formed or as people have tried to implement existing policies. In still more recent years the phrase "behavioral approach" has come into both political science and the study of business organizations. It has implied, among other things, a more extensive use of both the concepts and the data-gathering techniques of psychology, sociology, and anthropology. In place of analysis of printed or written records and of relatively unsystematic observations of behavior and loosely conducted interviews, we are now getting detailed systematic observations amounting at times almost to constant surveillance of key persons, verbatim transcripts of interviews and meetings, minute descriptions of behavior, and studies based on precise samples of individuals and incidents. "What happened" can be defined more objectively than in the absence of such procedures.

In addition, there is an increased emphasis on understanding before judging. Particularly in the study of politics the tendency to label the actions of politicians as "irrational" is decreasing, and the sense of responsibility for discovering the rationale that lies behind seemingly inexplicable behavior is increasing.

The point of view that has evolved has been stimulated by men with practical concerns. Chester Barnard, an articulate businessman, is certainly the progenitor of the mainstream of modern organizational theory in business. Political scientists with Government service have spurred developments in political science. While the concepts and tools of the disciplines of sociology, anthropology, and psychology have been borrowed to do this job, these heartland disciplines of the behavioral sciences have come onto the scene belatedly.

2. *The contemporary view.* An appropriate introduction to the contemporary view of policy formation may be some words I addressed to fellow psychologists in an attempt to distinguish policy formation from other matters with which I felt this process has been confused. I quote

from this address since the reader may benefit from knowing the precise terms in which I feel the old and new points of view may be distinguished from each other.

... there are three propositions that are essential for understanding the contemporary view.

The first is that moralizing and generalizing are not the same thing as policy making. Worse, moralizing and generalizing are often done to avoid the responsibility of thinking concretely in policy terms.

The second is that the policy process is a social process and not an intellectual process. Intellectual processes are part of the policy process, but ignoring the social processes produces misconceptions of a crucial nature.

The third proposition is that the intellectual process itself is not that which we connote when we talk about "decision making" in the study of human cognitive processes. There is every evidence that in complex policy situations, so-called "decision makers" do not strive to optimize some value, nor is the notion of optimization a useful way of ordering and analyzing their behavior.(2)

As I proceed, it will be noted that I will define the contemporary approach to the policy process both positively and in distinction from those conceptions with which policy formation has traditionally been confused. The reason for making both a positive and a negative description is that positive statements are usually plausible enough to seem self-evident unless it is made clear what alternatives have been rejected.

Traditionally, our greatest barrier to understanding how policy is formed has been the tendency to judge before understanding. The policy maker has been undervalued by both the public and the scholar because policies so often involve compromise and because the process appears inelegant to outsiders who are sure that some theoretically more sound approach is possible. All the difficult and distinctive aspects of policy formation lie outside of what is morally correct from the point of view of a given set of values, and scientifically correct for a given class of problems. It is the distinctive mission of the policy maker that he must mediate among conflicting sets of values and interests, judge what is possible as well as what is preferable, form judgments specific to the situation with which he is confronted rather than those which are correct in general for a class of situations, and finally balance each individual issue off against a wide range of other issues. It is not sufficient that he intend to do good, but that he invent some course of action that he can with responsibility count on to produce as much good as possible.

In recent times, processes of this sort have been called "decision making." The connotation of this phrase has proven unfortunate both in terms of the expectations that it raises and the fact that it deflects attention away from what is actually relevant in the policy process.

The term "decision making" implies a specific model of cognitive activity that assumes a single decision-making unit, with a single set of values or utility preferences, a knowledge of a reasonably full range of action alternatives and their consequences, the intention of selecting a course of

action of maximum utility, and the opportunity and capacity to make the appropriate calculations.

This concept of decision making is inappropriate or inadequate on several scores. The key element of inappropriateness is the absence of any single scheme of values or utility preferences on the basis of which a "best" solution could be evaluated. In any sizable group of people there must be some differences of personal values. Any course of action will benefit some people more than others, even if no one is positively disadvantaged. Hence, even people who have the same values will have different interests because they have different positions in the society. For this reason the very concept of a "best solution" is elusive. Welfare economists suggest that a system of side payments is possible to compensate those who are disadvantaged or even those who benefit less than others. Even if such a system of side payments were in effect, it is clear that there is no one policy that would be preferred above all others by everyone. Hence, it is possible to say that policies are "good" or that one policy is preferable to another, but not that any one is "best."

*The basic process by which the heterogeneity of values and interests in a complex group is solved is a process of negotiation, whether explicit or implicit.* Formal delegation of authority gives some individuals the opportunity to defer negotiation by putting policies into operation before a consensus has been reached. Such individuals also have the delegated authority to devise a set of values or preference scheme that will prove acceptable to the group. And some individuals will in fact misuse their power to enforce unpopular policies and defer negotiation excessively long, but they borrow trouble and pay costs of which they are only partly aware.

While the policy process does not conform to the model of decision making in this key fact of the lack of a unitary system of values or preferences, there are many other respects in which the model of decision making is insufficient.

For example, the decision-making model treats of selection among courses of action. It says nothing of the essential skill of the policy maker to invent, or to imagine courses of action that will be acceptable to a sufficient coalition of parties to elicit their support, and the ability to communicate the implications of a course of action so that the various parties will see where their interests lie. Furthermore, the notion of decision making assumes that the policy maker has the time and motivation to turn his full attention to the problem in question, whereas in fact he usually has the prior problem of how to allocate his time, attention, energy, and other resources among a host of competing problems. Thereby he pre-judges whether or not he can attend to a given problem, and what priority he will give it.

One point should be made clear. There is a definite role for the most formal and rigorous of decision-making models and devices in handling subproblems in policy formation. However, these subproblems—virtually by definition—share none of the characteristics of the policy problem *per se*. Hence, I now turn to the distinctive characteristics of policy problems.

I said previously that the policy process is a social process in which an intellectual process is embedded. It has already been treated as a social process, in that I referred to negotiation as the central mechanism whereby diversity of values is resolved. Negotiation is a social process involving assessment of other people's values and interests, skill in bargaining, and so on. Even this process is but part of a larger one.

We speak easily of "the problem" and of people's "interests," yet the problem is not a unitary phenomenon, nor do people ordinarily have any single clear interest.

The student of policy and even the individual actors are likely to think of "the problem"—let us say urban mass transportation—as a unitary problem. In fact, the range of relevant actors in the resolution of a policy issue will have a wide variety of problems that relate them to the issue, many of which bear no resemblance to each other except that all are tied to some common issue. If a mass transit line is proposed, a businessman in the suburbs may think of his difficulties of getting secretaries who live in the core city and need transportation to the suburbs, of parking for his executives, and of the local tax rate. The politician in the core city will be concerned with displacement of people, a changing ethnic composition of his constituency, and a changing tax base. Someone else will worry over the preservation of historical sites and the "traditional appearance of our city." And so on.

The policy maker has been aware of this range of "problems." Only recently has the student of policy been equally aware. The researcher analyzing the policy problem and the policy process must undertake the job of mapping the distribution of problems with which the various actors are confronted.

The stand a person takes on an issue, and presumably the "problem" that he thinks he has is assumed in some way to be dependent on his "interest" in the issue. With Pool and Dexter (3), I undertook a study of the making of United States foreign trade policy. We were told firmly that there was no mystery about the stand that business organizations take on issues like this: ". . . they simply look at the balance sheet, and act in their self interest!" In point of fact, neither the "self" nor the "interest" proved to be self-evident. Complex organizations may define the "self" in many ways. It may be the chief executive, the group of top executives, the stockholders, the firm as a profit center, the customers, the public, or the parent organization in the event that a firm

is a subsidiary of another company. Any one or combination of these "selves" is possible.

Any one of the individual "selves" has a complex of interests, many of which it is not aware, that may tie it to a given issue. One of the functions of those actors who are trying to influence policy is to formulate a version of the firm's interest that is plausible and will align it on one side or the other.

In the hearing on the Reciprocal Trade Act in one year of the mid-fifties, there was no necessary reason why the president of one large railroad should have testified in favor of quotas on the importation of both crude and residual fuel oils. It just happened that a representative of the coal interests persuaded the railroad president that the importation of residual fuel oil would lessen the demand for coal that his railroad carried. The crude oils were thrown into the plea because at that point the coal interests were in alliance with the domestic oil producers who were in competition with imported crude oil . . . This same man could just as well have been approached by any one of a number of port interests and reminded that he carried much more in the way of goods imported *via* several ports than he did coal.

An essential feature of the policy process is the active definition and redefinition of the problems and interests involved and the formulation and reformulation of policies to the point that a winning coalition is formed. This is a dynamic process, none of the elements of which can be taken for granted, and each of which must be the subject of empirical investigation as they change over time.

Since I am about to introduce more complexity into the situation, it might be appropriate to offer some reassurance that processes such as these are amenable to systematic study. I hereby tender such reassurance, and promise to tend to this matter later.

The discussion of the policy process to this point has proceeded almost entirely in terms of a single issue, as though policy makers live in a world in which they can attend to one issue at a time, and in which each issue can be separated from the ones that preceded it and the ones that follow. It is probably only in the time of a major war that the policy maker's world is so simply structured. And even then, the mature statesman is concerned with conducting himself so that he may be able to live constructively in the postwar world with the adversary he hopefully will conquer.

All events happen in a context of past, present, and future events. We have coined the phrase "the envelope of events and issues" to refer to those events and issues that must be considered as the context within which to analyze a given policy problem.

The simplest aspect of the problem has been hinted at previously: busy men do not have the resources to treat many issues as salient, and their

strategies for handling salient and nonsalient issues are probably quite different. Unfortunately, researchers have in the past tended to assume that, because *they* are interested in a problem, the person who has to deal with it finds it equally interesting and important. Since people are generally polite, even though busy, they tend, generally but not always, to answer a researcher's questions in his terms. It is one of the first tasks of the policy analyst to determine the context in which the individual actor is confronted with the issue in question, and the extent to which it is a problem about which he cares.

The person who fights every issue as though it were vital exhausts his resources, including most especially the patience and good will of those on whom he has to depend in order to get things done. Therefore, it should be considered neither surprising nor immoral that, on issues of low salience, a sensible individual may take the occasion to trade off his immediate interest for future good will, or, if his resources of time, attention, and so on are severely taxed, he may decide to "sit this one out."

Hence, in order to understand or predict the way in which a given issue will be handled, one must determine the salience of an issue relative to other things with which the individual has to cope; and this is seldom self-evident.

We have not yet dealt with the influence of past and future events. The time-binding nature of the policy process manifests itself in many ways. During one session of Congress in the early fifties, the Eisenhower Administration had put considerable demands on the chairman of a relevant committee to secure the passage of a bill in which the Administration was interested. As a result of the amount of good will—and committee time—that had been expended on the previous bill, the Administration was reluctant to, and did not ask the chairman to, hold full hearings on the Reciprocal Trade Act. In effect, what had happened before foreign-trade legislation came to the committee was decisive in determining how the bill was handled.

And, since policy making is by definition the setting of a course to be followed in the future, it is redundant to say that consideration of future consequences is part of the "envelope" that effects the policy process. However, the policy maker's view of the future world—if he has wisdom—is different from that implied in concepts of decision making, problem solving, or goal seeking. All these concepts imply a discreet sequence of events with a single definite point of termination. Serious policy making does not involve such decisive resolutions in politics. Each of these events redefined the terms in which an ongoing struggle was conducted.

The experienced policy maker knows that, as he resolves one issue, he is posing others. Accordingly he will ordinarily hedge his victory to give himself room for maneuver in the future. One of the ways in which

he does this is not to force on the losing coalition terms that will make it unduly difficult to work with them in the future. He recognizes that in the course of working on one issue he must preserve both the process and the institutions for dealing with such issues. The businessman hesitates to alienate the subordinates on whom he is dependent. The Congressman, both in his own interest and in consideration of his commitment to his responsibilities, tries not to destroy his working relationship with people who oppose him on individual issues. And so on.

These types of considerations enter into the behavior of all responsible men. For the legislator, however, they pose special problems: a very large portion of what he does and says is public; and his range of responsibilities is very broad. Because the popular notion of how policy is formed tacitly assumes that only one issue is involved, that only one interest is involved, that there is a clearly correct answer (despite the heat of the debate), and that the legislator's job is solely to deal with that issue, the visibility of his behavior and the range of his commitments have led others to regard him as spineless (he compromises) or witless (he is behaving irrationally).

The failure of other parties to recognize the Congressman's range of involvements led many people, in the Bauer, Dexter, Pool study, to regard his reaction to foreign-trade legislation as reflecting either ignorance or lack of responsibility. Many men, well committed to a liberalization of our trade policies had individual constituents who were "hurting" under the impact of imports. Our observation was that if the Congressman mentioned this latter circumstance in his conversation with a lobbyist for the Reciprocal Trade Act, the lobbyist assumed this man had no comprehension of the national interest, was moved by irrelevant considerations, needed a course in economics, and so on. Frequently, what the Congressman wanted was some suggestion as to what to say to his constituent to explain that he was sympathetic to his plight even though he was going to vote for the Reciprocal Trade Act.

However, the fact that other responsible men are often able to conduct their affairs in privacy does not mean that they act conspicuously differently. It is often considered that businessmen are in some way more "rational" than politicians. I therefore offer the following excerpts from a recent book on how business *actually* make decisions to invest abroad:

In a business situation one quite often does not "decide" in Webster's sense of the word. One is forced by previous commitments to act in a certain way. . . .

In this book the "decision" will be used without any implications of a conscious or deliberate intellectual process. We shall use this term simply to refer to the fact that individuals or organizations by following one course of action forego thereby other courses of action. By "decision process" we mean the continuous dynamic social process of mutual influences among various members of an organization, constrained by the organization's strategy, its resources, and the limited capacity, goals, and needs of its members, throughout which choices emerge. . . .



Quite often decision does not make sense unless the observer takes into account events and factors that occurred before the problem was presented, or that are happening concurrently in other remote areas. (4)

The fact that Aharoni's terminology is similar to mine is no mystery. We have been colleagues. It is relevant that he found it appropriate for describing and analyzing such a reputedly highly rational process—the decisions of businessmen to invest their money in a foreign country. The model I have outlined seems to be appropriate to any situation in which men tackle complex problems.

3. *The process summarized.* Let me now return to summarize what was said, to indicate its implications and potential, and what can be done to realize that potential.

In the past, the failure to understand the nature of policy problems and processes had led to improper analysis and research on both the problems and the process, and a mistaken evaluation of the process, of the results of the process, and of the persons involved in the process. Without glossing over any of the deficiencies of any of the elements in the policy process, we may still conclude that they have been generally undervalued because they have been judged against inappropriate criteria. Furthermore, this use of inappropriate criteria has deflected attention away from what actually goes on, and thereby lessens the possibility for improving the process.

I began by drawing a distinction between the policy process and those things with which it has most frequently been confused: moralizing, which is the statement of a preferred state of affairs; theorizing, which is concerned with the best solution of a class of problems; and decision making, which in some situations is a cognitive process for taking the preference scheme involved in moralizing, the generalized knowledge involved in theorizing, and the characteristics of the particular situation, and from these deriving a determinate best solution to the particular problem.

The difficulties of the decision-making model are dual: the requirements of the intellectual process involved with the model cannot be met, and the intellectual process can scarcely if ever be isolated from a network of social processes in which it is embedded.

As an intellectual process, decision making promises a "best solution" only if we have one single scheme of preferred events. Since there is no derivable single preference scheme for large groups of people, the notion of an unequivocally "best" solution has to be abandoned except in cases so obvious as to be trivial. (For example, if a particular decision involved the survival of mankind, it would be—in most cases—specious to argue that those people who wished to die ought to be consulted.) Since there is no derivable single preference scheme for large groups of people, the problem of multiple value schemes and multiple interests had to be

worked out either by explicit negotiating, or implicitly by delegating authority to someone who, in effect, anticipates the outcome of a negotiation. A third logical possibility is, of course, that the authority be usurped by someone who wishes to impose his own preference scheme. This is possible within limits of time and of the spread between his preference schedule and that of others. Any of these solutions involves a social process that is external to a decision-making model.

Also external to the decision-making model is the invention of viable courses of action among which to choose, the phrasing of proposed courses of action in terms relevant to the participants, the spelling out of the interests of the participants in terms relevant to the proposal, and the appropriate rephrasing of the proposal and the issue—or the invention of new courses of action—as the evolving process dictates.

Beyond this, both the social and the intellectual aspects of a given policy issue are embedded in the context of other issues, past, current, future, public, organizational, and personal, and in the lives of institutions whose main purpose may be only loosely related to the issue. From the point of view of any given actor, any given issue is one among many of the things to which he has to attend. The perspective in which he sees it, the resources he can bring to bear, the information available to him not only must be shared with other issues, but are to a large extent a function of his behavior in times past when it may have been beyond his conception that such an issue would ever exist.

4. *Applications of the model.* This is not to say that all is chaos, or merely that everything in the world is related to everything else. Because the real world is so complex, men and institutions find ways of simplifying it. The virtue of having a complex map of a complex process such as policy formation is that one can decide more intelligently what to ignore, and what to attend to. The point of view outlined here has been used to illuminate such problems as: Federal budget making (5), the American business community's involvement in the formation of foreign-trade policy (6), businessmen's decisions to invest abroad (7), policy making in New Haven (8), the way in which business institutions perceive or fail to perceive marketing opportunities abroad (9), the Congressman's role and his participation in foreign-trade policy (10), the way in which businesses scan business environments (11), capital budgeting in business (12), a community highway-location controversy (13).

In all the above instances, the researchers have tailored their view of the complexity of the process under investigation to what was feasible with their finite resources. All, however, have had a number of elements in common: a concern for observing what *ought* to be done; an insistence on understanding the picture of the situation that the people themselves involved with the problem have; a withholding of judgment as to whether

the established view of what is "rational" is necessarily the preferred way of doing things; a concern for the personal and social contexts in which people go about their business.

The practicality of many findings is already established. It is reputed that the analysis that Bauer, Pool, and Dexter made of the concerns of protectionists on foreign-trade policy was influential in shaping the Kennedy Administration's approach to the renewal of the Reciprocal Trade Act.

The study of the capital-budgeting process in large business organizations made by Bower revealed the following: While it is ordinarily considered that the top corporate committee concerned with passing on requests for capital investments is engaged in making decisions for the allocation of a scarce resource—money—it turned out that virtually none of the companies involved had any shortage of capital for the requests brought to them during the period. The executives requesting capital investments never made such a request unless they were practically certain that it would be approved. They had learned the decision rules of the capital-budgeting committee to the point that they offered only choices sure to be accepted. Hence, the committee was not offered enough opportunities for consideration to exercise judgment for the full utilization of available capital. The realization of this state of affairs makes it possible for men in such firms to take action against a problem they did not know existed.

Aharóni's study of foreign-investment decisions reveals that existing incentive programs used by underdeveloped countries for attracting foreign capital are irrelevant to the way in which businessmen think about such investments. Tax benefits for foreign investors, for example, are based on the assumption that the investor will make that investment that promises the highest rate of return on the money invested. This assumption is based in turn on the further assumption that money is the scarce resource that guides businessmen in their decisions. Actually, the men he studied seem to regard executive time and energy as their most scarce resource. The incentive program that he proposed as a consequence of his research was designed to reassure executives of the reasonableness of the risk of investing in the underdeveloped country, to aid them in investigation of the potential for *them* in that country, and to explore whether or not it would be possible to operate in that country without an excessive drain on the time, energy, and attention of their existing pool of executives. It is illuminating that in both the previous cases close observation of what went on upset one of the oldest and most "self-evident" propositions of the economist, that the key activity of business is what to do with money as a scarce resource. In neither situation did the scarcity of money ever get to the center of the decision stage. This is not to deny that money is a scarce resource and sometimes

*the* scarce resource, but merely to point to those situations in which scarcity of money was traditionally assessed as the central element when, in fact, it was not.

Such examples as those listed are evidence of our enhanced ability to analyze policy problems in a manner relevant for action.

At the same time that our ability to analyze action problems is improving, we are also building up a body of generalized knowledge. The "point of view" or "conceptual scheme" that I presented above is a distillation from several decades of work by many investigators. It is, of course, highly general, and tells us, more than anything, what sorts of things to look for and at, and to avoid certain preconceptions that will get in our way. However, many findings of a much more specific nature are accumulating. Some are such as these: Businessmen are more likely to respond to information that indicates the presence of a threat to their position than to information about an opportunity to increase their earnings, even though responding to the opportunity will have at least as great an expected payoff as does avoiding the threat. (This does not correspond to a straightforward notion of rationality.) Likewise, business executives, when confronted with opportunities for investment, tend erroneously to act as though they were investing their own money. This is "erroneous" since they refuse risks that would be rational from the company's point of view in the light of its ability to sustain individual losses. What would plunge an individual into bankruptcy would be a sensible and tolerable loss for the company with its much larger ability to absorb single losses while following a policy that will pay off in the long run. Or, different people define their roles in different ways, and consequently behave differently. Thus, a "young man on the make" who is an export manager of a large company will assume that he will move on from that job. Consequently he will seek out a wider range of information than will an old timer who sees the job of export manager as his final destiny.

Because of the complexity of some problems, people often have no available formal model for their solution, and many develop what we have long called rules of thumb, now given the more elegant designation "decision rules." Researchers at the Carnegie Institute of Technology have been concerned for some time in understanding the way the human mind copes with problems that are too unstructured to fit formal models. Apart from the theoretical work of Herbert Simon and his colleagues, their concepts and methodology made it possible for Clarkson to learn the decision rules of an investment banker to the point that he could devise a computer program that predicted the future content of an investment portfolio managed by this banker with a very high degree of accuracy (14).

Examples of this sort could be extended at length. My purpose here, however, is to illustrate the fact that quite specific propositions have been developed—much more concrete ones than the broad conceptual scheme I have outlined above.

Though I have only lightly touched upon methods—assuming that the reader is satisfied that they exist—the activities that I have been discussing have helped to develop a number of methods of data gathering and analysis that are appropriate for use under a variety of circumstances and that offer fruitful opportunities for further exploitation. These methods enable us to overcome the disadvantages inherent in over-reliance on the use of survey-interview techniques; these techniques may at best be a second-choice methodology dictated largely by lack of available funds and of institutional settings permitting sufficient resources to be brought together in collaborative activities, not only for the collection of data but also for its analysis.

These developments include such things as around-the-clock observation, verbatim tapings of conferences simultaneously with observation by trained researchers and the development of the time dimension of the policy process by systematically designed studies at several points in time while some elements of policy processes are kept under relatively constant surveillance.

Much of these new approaches focuses on cognitive processes, the ways in which people see things, and the ways in which they think when dealing with problems. Techniques have been developed based on laboratory work in the psychology of cognition and the methods that linguist have used for studying languages to uncover the ways in which people categorize those events of their world that are of importance.

A so-called "protocol method" has been developed for studying people's thought processes by having them think out loud while they are working on problems. The "decision rules" inferred from such protocols are then validated by building a model for computer simulation to test whether the model produces the same results as the human.

However, the existence of appropriate methodologies does not assure that they will be used. Part of this difficulty lies in the fact that some technologies are by no means universally known, and researchers tend to use tools with which they are familiar. But a deeper problem is that the appropriate methods are relatively more expensive and they require an adequate institutional setting and scale for inter-disciplinary application.

The generally least recognized feature of the contemporary view of the policy process is that it makes applicable much of the systematic knowledge and methods of the behavioral sciences. A great deal of systematic work has been done on negotiation, on role analysis, on communication in interpersonal relations, on measurement of personal values and beliefs, on the process of cognition ranging from the way in which

man scans his environment to strategies and decision rules he develops for dealing with complex unstructured problems. In varying degrees methods have been developed for studying such processes under limited but controlled experimental conditions and under the most natural of complex real-life situations.

A good deal of research has been done on "decision making" in the more literal sense—both normative work as to how decisions should be made, and descriptive work as to how they are made. If the notion of decision making were used to represent the complex phenomena about which I have been writing, then our attention would be directed to the wrong body of systematic work. The conceptualization of a proper model of the process is important not only because it directs us to the proper body of systematic work, but also because it directs us to ask the proper questions of those systematic areas of investigation. For example, it is clear that the "process of negotiation begins before the parties confront each other physically. And, negotiation takes place implicitly when a person "exercises his authority" in order to avoid or defer explicit negotiation. It is clear that to understand both these conditions we need to understand the process of negotiation as it is carried out vicariously in the head of one man while his own imagination creates a surrogate for the person with whom he anticipates negotiating.

It should be noted that no one piece of research done under these rubrics in any way requires the adoption of a total model or a total set of procedures. One does not have to research a policy problem from beginning to end and in its full complexity in order to learn something worthwhile. The full offering of concepts, findings, and methods constitutes an armamentarium from which one can select what is relevant.

There are opportunities for a fairly broad range of practical results. They include analysis of specific policy problems on a basis more useful to the man who must take action and generalized knowledge of the sort enumerated above. Further, we may anticipate that recognition of the social context within which intellectual process are brought to bear on important problems will enable us to understand more realistically the limits of performance we can expect of men and institutions, to free the intellectual process from the interference of the social matrix in which it is embedded, and to facilitate the social process itself, thereby making negotiations more efficient and equitable.

5. *Suggestions for action.* While the potential for a wide variety of practical contributions seems well established, the achievement of these potentials is quite another question. Unfortunately neither behavioral scientists, educational administrators, nor foundation executives have much or any experience in developing areas of applied behavioral science. Nor are they accustomed to the budgets that should be involved. This has made the establishment of the programs that I have proposed very

difficult. Consequently, the research methods adopted by behavioral scientists have not always been the best, nor have the results of their work been sufficiently documented to make it feasible to determine with precision how much work is being done from the points of view presented here. It is a way of thinking that is well recognized by some dozens of well-known investigators, and is accepted in part by many others who may not be aware of or accept the full version. However, these adherents are spread throughout the United States, with only a few conspicuous clusters: political scientists at Yale, political scientists and students of business at Carnegie Tech, students of business at Harvard Business School, general behavioral scientists and students of administration such as Harold Guetzkow, James March, and Richard Snyder who have recently gone to the Irvine campus of the University of California, and other individuals or small groups. Predominantly they are people interested in the substantive problems of Government and business, and occasionally of non-profit institutions such as hospitals (15). However, the concepts and particularly the methods more often used tend to be developed more by sociologists, psychologists, and anthropologists. These latter behavioral scientists, as indicated earlier, have been relatively slow in coming to studies of the policy process.

A significant constraint is that there is no single place at which the effort is of sufficient magnitude and scale to develop the potential of this contemporary viewpoint and to achieve the practical potentials that it offers. Authors of other papers in this series, who have dealt with problems of developing areas of applied natural sciences, have stressed the need for a fairly sizable body of ongoing work so that concepts, methods, and empirical findings can be tested and revised rapidly; for fairly large groups of people interacting with and learning from each other; and for these efforts to be guided by relatively coherent doctrines.

The coherent doctrine in policy formation exists in reasonable form, if the reader is willing to accept the version I have presented. Hence, the principal need is for several centers of activity that are institutionally designed to provide the scale and continuity of interdisciplinary effort in research on policy processes. Such centers may be best located in large universities with a variety of professional schools and groups with interests in substantive problems—schools of public administration, business, architecture and planning, urban affairs, engineering, education, public health, and so on. Activity should be organized around a nucleus of perhaps a half dozen senior behavioral scientists with the required conceptual and methodological background, and dedicated to exploration of action problems. Their task would be initially to establish a mutuality of interest with the people who have the substantive problems, and thereby begin building the volume of research that is required to exploit and develop the potential of the approach I have outlined.

The principal need is a sharply focused effort to assemble the necessary support and to develop in more detail the design for such centers. These must, however, be achieved in a manner that would not make the centers susceptible to pressures for unbalanced approaches or divert them from appropriate attention to some of the more difficult and hence longer-term research questions.

### **B. Social Indicators**

On February 6, 1967, Senator Mondale of Minnesota introduced on the Senate floor a bill entitled "The Full Opportunity and Social Accounting Act of 1967." As part of his explanation for what he proposed, the Senator has this to say:

The fact is that neither the President nor the Congress nor the public has the kind of broad-scale information and analysis needed to adequately assess our progress toward achievement of our national social aspirations.

In making this statement, Senator Mondale was reflecting both a new and an old concern. Since the formation of the Republic we have taken decennial census, and subsequently we have added other statistical series that have been used to assess the state of the society and as a basis for planning for the future. In this regard, the issue is an old one. However, in recent decades planning for future needs has increased, and with it a demand for more and better social statistics.

1. *Recent background of the movement.* A landmark in the development of planning and control in American society was the passage of the Full Employment Act of 1946. To implement the intent of this act, a series of economic indicators was introduced which, together with a generally shared model of the American economy, has become a useful instrument in achieving economic stability and growth in the past two decades.

Progressively it has been recognized that, despite the usefulness of the economic indicators, they have limitations. These limitations are in part technical in that they are not always adequate measures of what they purport to measure, but more importantly it has been recognized that economic measures alone are not adequate criteria of many of the things about which we are concerned. In the past few years, concern over the Great Society has been coupled with the notion of the "quality of life," and interest has developed in broader statistical measures to assess the state of the noneconomic aspects of life. It is in this respect that the call for a system of "social accounts" by Senator Mondale reflects a new, recent trend of thinking (16).

The most dramatic manifestation of this concern was initiated by a directive in the 1966 Presidential Message to the Congress on domestic health and education. In this speech President Johnson announced that he had asked the Secretary of Health, Education, and Welfare "to estab-



lish within his office the resources to develop the necessary social statistics and indicators to supplement those prepared by the Bureau of Labor Statistics and the Council of Economic Advisors. With these yardsticks, we can better measure the distance we have come and plan for the way ahead."

As a consequence of this request from the President, a panel of statisticians and behavioral scientists has been formed under the direction of the Department of Health, Education, and Welfare. This panel has set for itself the objective of drafting a trial Presidential social report of the Nation to parallel the economic report. This report will attempt to assess the extent to which we have created a society that permits the realization of our full human potential. While it is premature to guess how successful this venture will be, the nature of its intent is clear. In addition to being an attempt to see how adequately this goal may presently be met, this effort is also intended as a test for the adequacy of our present data series. Inevitably new series will be proposed, but this exercise will help establish priorities.

Other concrete manifestations of this accelerated interest in social measurement in the United States are: the establishment of a program to review our measures of social change by the Russell Sage Foundation, a proposal by the National Commission on Technology, Automation and Economic Progress for measures of social change that extend beyond our measures of economic change, the publication of our own book, *Social Indicators*, mentioned previously, and an issue of the *Annals of the American Academy of Political and Social Science* in progress under the editorship of Bertram Gross.

This latter volume will cover our knowledge of and the adequacy of our measures of various aspects of American society. The following topics are included: political participation; freedom from discrimination; civil liberties; reduction of poverty; employment and leisure; learning and education; health and well-being; delinquency and crime; the urban environment, general; the urban environment, New York; the natural environment; culture and the arts; the mass media; and the individual and group values.

On the international scene, the Secretary-General of the United Nations issued, in 1965, a report, *Methods of Determining Social Allocations*, devoted to this topic, calling for "the development of a comprehensive set of criteria that will take account of both economic and social considerations, not by forcing one kind into the mould of the other . . ." Indications of new interest have come from a number of specific countries: France, Britain, Canada, and Israel, to my knowledge.

The intent of this movement is not to set one type of statistical series off against another, but, in the word of the United Nations report, to "integrate them at a higher level of abstraction." This will involve both the

measurement of new phenomena, and more adequate measures of phenomena that we now measure less well than we can.

Intuitively one would relate the interest in Social Indicators to the recently popular interest in "data banks." While the two interests overlap somewhat, their foci are quite different. One is concerned with storage and retrieval of available data (data banks); the other is concerned with a better understanding of data needs, with the specification of the required data, and the devising of methods of gathering them so as to meet the needs more precisely. The two interests could be pursued independently of each other. However, the availability of computers for storage and retrieval of data will no doubt facilitate a system of Social Indicators.

2. *Measurement of past performance and future potential.* Professor Bertram Gross (17), in probably the most comprehensive extant statement of the aspirations one might set for a system of social indicators, proposes a system for social accounting that would fall into two broad categories. The first of these categories (18), "system performance," would include measures of the extent to which at a given point in time we have been able to meet the values and aspirations of our citizens. The other broad category, "system structure," would include measures of the society's capacity to perform its functions in the future.

Measures of performance would include: our present measures of economic performance, measures of the equitable distribution of economic benefits throughout the society, of the educational and economic opportunities we have afforded our people, the environment we have created for them to live in, the cultural opportunities we have offered, the extent to which we have created a peaceful world, the level of health of the Nation, and so on. Any state of affairs that people value and aspire to is a potential candidate for inclusion in this list, as is any state of affairs they would like to avoid.

The following passage from Senator Mondale's speech is illustrative of some of the "performance" data. It is suggested that we need to evaluate how well we are serving the needs of our citizenry.

How many Americans suffer in the squalor of inadequate housing? How many children do not receive educations commensurate with their abilities? To how many citizens is equality of justice denied? How many convicts in our penal institutions are barred from rehabilitation that would allow them the opportunity to re-enter the mainstream of life? How many physically handicapped and mentally retarded are unable to get training to achieve their potential? How many individual Americans are denied adequate health care? How many are breathing polluted air? These are some of the possible indicators that might be considered in the social accounting. (19)

One of the standard criticisms of the welfare state has been that present benefits are skimmed off "at the cost of future generations." The concern is, of course, a legitimate one. And it is for this reason that Gross would parallel measures of present performance with measures of future capacity to perform. Such measures would include presently conventional measures

of the strength of the economy, less conventional measures of the state of science and technology, the state of skills and knowledge in the population, public health, the extent to which our institutions make it possible for all members of the society to make their potential contribution, and so on.

It is clear that many of the things one would want to measure would have to be entered on both sides of the ledger. Education can be viewed in some part as a benefit to the citizen and in some part as a future economic asset to the society. Crime, drug addiction, and delinquency are both indications of the failure of the society to perform as well as it might, and a liability for future performance. Health, also, has such a dual status. This circumstance is a stimulus for a better conceptualization of what interests us.

A plea for "better conceptualization" almost certainly has an un-earthly, abstract, and academic ring. Happily I can report that this is exactly what has been going on successfully in the National Center for Health Statistics of the Department of Health, Education and Welfare (20). Until the mid-fifties, our primary concern was with our ability to keep people alive at various ages; *mortality* was the relevant criterion of health. Since then, considerable attention has been devoted to *morbidity*—or departures from health of a nonfatal sort. But nonhealth is not as clear-cut as death. It may be thought of as the absence of a detectable disease, "disease" being defined as what a medical doctor regards as a malfunctioning of the organism. It may also be defined in terms of the individual's ability to continue to perform with some degree of effectiveness in his various roles. Several of the concepts of disability or nonhealth employed in the Health Interview Survey, conducted monthly by the Health, Education and Welfare, have borne on the ability of the individual to perform in his essential roles—as a worker, housewife, student, mother, father, and so on.

The immediate physical condition of a person is a matter of concern for his own state of comfort, may have economic consequences because of his need for care and medication, may present a threat to others if his condition is infectious, may serve as an indication of our ability to conquer certain diseases, and so on. However, if we are concerned directly with the individual's ability to contribute to the society, or conversely with the effect of sickness in general, of a given disease, or of a specific epidemic on economic production or on the conduct of the day-to-day business of the society, then a measure of the extent to which people are able to perform in their roles is the most relevant criterion of health we can have. At a certain stage of its development, cancer is less likely to restrict role performance than is the common cold.

3. *Assignment of value.* What should be measured? How should it be measured? Neither question could or should be answered neatly at this

point in time. All we can do is suggest the sort of criteria to be invoked, and the direction in which things are likely to go.

The data we want obviously are dependent on the goals we set for ourselves, on the type of people and the type of society we are. A society primarily bent on achieving military power would be interested in measuring different things than would a society bent on maximizing religiosity, aesthetic experience, or material comfort. Therefore, we need to know the values of the members of our society, the programs of national goals that *ought* to represent attempts to serve those values, and the interrelationship of the programs and values.

(It will be noted that we have doubled back on our concern with policy problems, since a better knowledge of the values of our people would enable those persons who have to devise viable policies to do so more efficiently. I use the word "efficiently" advisedly since I want to avoid the image of policy makers being made over into passive ventriloquist's dummies, mouthing the results of public opinion polls. I assume that policy makers *must* be inventive, *must* see that the presently expressed preferences of the public are an imperfect guide to the courses of action to be set in a complex situation. I would further assume that policy makers do and will use their knowledge of the public's values for guidance, that they will use their delegated powers often to inaugurate programs whose worth must be demonstrated to those who cannot be convinced in advance. At the same time, I still further assume that a better anticipation of what will prove acceptable will enable policy makers to minimize the proportion of misguided missions that they launch.)

Knowledge of people's values and aspirations is a key to planning and control. It tells us on the one hand what our people want, and on the other the sorts of programs they will support and/or tolerate. The study of values and aspirations can be technically difficult, and politically sensitive. Yet, as difficult and sensitive as it is, it continues to be done, no matter how inadequately. For example, politicians know that opinion poll data showing a low level of public support for the space program is not an urgent mandate for dismantling the National Aeronautics and Space Administration. At no point will knowledge of such values and aspirations be a clear guide to a specific course of action. Yet better knowledge of people's values can give us an improved basis for judgment on many issues.

In the market place, the conventional and probably proper guide to pricing of goods to be sold is the amount that people will pay for them. It is characteristic, however, of many public investments that their products (for instance, weather forecasting) can be shared by an untold number of people; thus, the "value" of such goods and services is a function of the number of people who will benefit from them. But in the

absence of a direct measure of value in terms of the utility of public goods and services to those who use them, the current practice is to evaluate a public investment in terms of its cost. Economists agree that utility cannot be measured solely in monetary terms (at a minimum, a dollar is worth less to some people than to others). Hence, to the extent that we can evolve measures of utility that are a direct reflection of the benefits people perceive themselves as receiving, the more nearly sufficient basis we will have for evaluating public investments whose value is dependent upon the number of persons benefiting.

Again, we are not proposing the tilling of unplowed ground. Present methods of program evaluation often involve research on the benefits conveyed. The Poverty Program, for example, is presently the object of such evaluations. The point I want to make is that, as we develop increasingly more adequate measures of people's values and expectations, we will also be able to develop more appropriate measures of benefits. (We would judge differently the effect of a physical fitness program on a boy who wanted to become an engineer than we would its effect on a boy who wanted to become a professional athlete.) The combination will enable us to plan better and evaluate better.

4. *Cost or investment in the future.* Another issue on which more appropriate measures will aid planning and evaluation is the extent to which an expenditure should be regarded as a *cost* or as an *investment*. For example, by and large, expenditures on education have been regarded as a public cost producing a private benefit. However, a business firm that builds a plant will regard this as a capital investment that increases its assets. Corporate accounting draws a reasonable distinction between money consumed in the form of current costs and money invested to yield some future return. Is an investment in plant equipment to be treated differently than an investment in training? The idea of Federal capital budgeting has been around for some time. It would make more sense if we had a better basis on which to distinguish between an investment and a cost, and what proportion of a dollar spent should be allocated to each.

Our concern with the educational level of the population illustrates both a technical inadequacy in not measuring what we want to measure, and a measure that perpetuates the confusion between costs and investment. As we plot the educational levels of our people over time, or the comparative education of various groups in the population, we view the data from two perspectives—as an index of the benefits we confer on our people, and as a measure to capacities of these people to share and produce culture, to perform productive work, and so on. From the first point of view—benefits conferred—we are becoming aware of the imperfections of this measure. Eight years of education in a segregated school is not the equivalent of the same term in an integrated or in an all-white

school. Further, as a measure of the capacities of the population for productive work, number of years of schooling achieved is an exceedingly poor measure of a man's present capacity to perform the tasks of the work force at a given period of time. It is probably an even worse measure of his capacity to learn new knowledge and skills.

Undersecretary of Health, Education and Welfare, Wilbur J. Cohen, has had this to say:

When we survey the voluminous, yet unsuitable data now available for assessing the products of our education, we must conclude that practically none of it measures the output of our educational system in terms that really matter (that is, in terms of what students have learned). Amazement at the revelation of the tremendous lack of suitable indicators is almost overshadowed by the incredible fact that the Nation has, year after year, been spending billions of dollars on an enterprise without a realistic accounting of that investment. (21)

There is an established technology for making direct measures of knowledge, ability, and achievement. While there has been controversy over the use of tests devised by psychologists, the fact is that they are in no way inferior to the criteria whereby students are promoted in and graduate from schools that they attend. Properly used, they can be much more adequate measures of the present status of the population's skills, knowledge, and so on. Such measures, gathered on a sample basis, can give us a far more precise estimate of the capacities of the U.S. population on relevant dimensions than we can gauge by records of past educational history. (The value of educational history itself becomes less relevant, as approximately half of education and training occurs outside the formal educational establishment.)

At this point in time, a considerable number of studies have been conducted, applying various measures of ability on national subsamples, usually of children. The ongoing Carnegie Corporation study of quality of education being conducted by Ralph Tyler is the most current example. Such measures are entirely feasible, and, since they must be conducted on a sample basis, are not excessively expensive.

Most relevant, they focus our attention on the fact that we are interested in our citizens as a national resource as well as beneficiaries. More appropriate conceptualization of what we are interested in, and proper methods of measurement such as we have mentioned apropos of health and education will enable us to make more sensible distinctions between those proportions of health and educational expenses that should be regarded as costs and those that should be regarded as capital investment.

5. *The sample survey.* A distinctive aspect of both the new statistical series and new methods of measuring established phenomena is that, increasingly, the preferred instrument for gathering the new data is the sample survey. Sometimes this is motivated by the need for speed (the monthly survey of employment), by costs (the subsamples of the national census), but more interestingly by the fact that people rather than

records are the proper source of information. No extant set of records was an adequate source of data on unemployment. Only a properly drawn sample of adults of employable age could supply the information about their employment status and intentions. The argument for testing the skill, knowledge, and ability level of our population directly has been made above. Similarly, only the people involved can tell us of the effect of their health on their ability to carry out their daily responsibilities.

There are interesting practical aspects to this circumstance. Gathering of data from people places demands on their time, energy, and patience. Data to be gathered from people must be gathered on a sample basis if only for the simple reason that no one individual can devote his entire life to supplying information.

Sample surveys can provide more rapid information, because the gathering and processing of data is enormously less extensive, of course, than for a census. Samples properly drawn, with correct measures for ensuring completion of the sample design, are conceded to be at least as accurate as a census. Costs can be quite modest relative to the value of the information acquired. In a country with a population as large as that of the United States, the sample survey is especially attractive since the unit cost of information goes down with the size of the population from which it is drawn. (The adequacy of a sample is a function of its own size, not of its size relative to the population—for any reasonably sized population.)

Of course, any proposal for additional social measurements must, in the light of present concerns, face up to the extent to which many responsible people are appropriately worried over threats to personal privacy. This consideration has been accentuated in connection with the recently proposed national data bank. Anxiety over invasion of privacy takes many forms. In this context the most appropriate version of this concern is that data files may be turned into dossiers to be used against individuals. Critics of the proposed national data bank express fear that it will be possible to "know everything about everybody," and that someone with access to that data bank may use information for political control or blackmail in many forms.

I happen to believe that the fears concerning the national data bank may be excessive. However, what is important is that *the various proposals for gathering more adequate social indicators may actually reduce the cause of such anxiety*. Most of the measures would be made on proportionately small samples of the population, in which individuals need not be identified. Mainly, they would be one-shot studies with no need to return to the people in particular samples. No one person would be likely to be drawn into more than a few samples over a reasonable period of years, so that, even if his identity were preserved, files could contain only a limited amount of information about him. Some data series, such as those pertaining to opportunities for educational and occupational ad-

vancement, would have to be gathered on a longitudinal basis, with a sample of people followed for many years. Even if this were a very large sample of several hundred thousand people, it would be a small proportion of "everybody."

There can be little argument against better data as a tool for planning and control. Costs would be moderate. Even quite an elaborate survey, as elaborate as any that might be required to make a national estimate on any of the variables mentioned above can be made for a few hundred thousand dollars. Simpler national estimates can be made for tens of thousands of dollars. Costs would depend on the frequency with which measures are made, and the extent to which one wishes to measure subgroups in the population. The number of series, the frequency, and size of studies can be advanced at any pace that proves desirable—hopefully as existing series prove their worth.

Let us consider some of the matters that should concern us. The most legitimate obvious worry is that widespread data-gathering might become an imposition on the population of the United States, and that the pursuit of a worthwhile cause might turn into a senseless fad, in which data are gathered indiscriminately and to no good purpose. In answer to this concern I would have to reply that I assume we are dealing with sensible men, who in turn will be scrutinized by other sensible men.

The use of sample surveys reduces vastly the numbers of people needed to gather data on given topics, and it is therefore probable that any reasonable program of expansion of our statistical series can be carried off without substantial burden on the populace.

Probably a more realistic consideration is that too few statistical series will be experimented with, that these few will be prematurely frozen so that we will be burdened for decades with poorer data than we ought to have. Provision should be made deliberately for experimentation with measures of many concepts. In connection with measures of the values and expectations of the citizenry it is especially imperative that a number of ways of going about the task be tried.

The utility to the policy maker of such data series as are being proposed seems self-evident, or at least has been spelled out by many policy makers themselves. These data will give more precise means of estimating the state of the relevant dimensions of the society, and most especially of noneconomic aspects that are presently weakly represented.

I have put less stress on the scientific worth of these data than on their practical utility. However, if behavioral scientists are to develop adequate understanding of the functioning of large-scale social systems, they must have data on such systems. In turn, it is to be assumed that this comprehension of our society and of social systems in general will have practical benefits. Without doubt, the economists are decades ahead of other behavioral scientists in the adequacy of their formal models, such as those used for controlling the national economy. While economists were active



in many ways at earlier times, their work was also favored by more adequate data series. Statistics gathered for purposes of Government or business administration are generally more suitable for economists than for other behavioral scientists with other concerns. This is not surprising; a commercial society keeps more adequate records of its economic transactions than it does of noneconomic data.

General models of social systems exist, as do models of the American social system. It is safe to say, however, that all of them require considerable development and refinement before one could use them to plan developments in the noneconomic sector with any degree of precision. On the whole, the choice of statistical series for the immediate future will have to be based either on a reasonable consensus that certain phenomena are "important" regardless of the social system model one might prefer, or that individual series—e.g., equality of employment opportunity—are valuable for their own sake.

The "social accounting" model proposed by Bertram Gross (22) can be used for the tabulation of the flow of "good" and "bad" events—increases in things people value versus increases in social costs—without commitment to any one dynamic model of the society, or for that matter without the notion that these "good" and "bad" events can be compared on a common yardstick. However, the existence of better data against which to check common denominators of values, will make it possible to have better estimates of the comparative worth of such models and measures.

### Summary

In closing my discussion of the study of policy formulation, I have assessed the circumstances and prospects for the development of that area. In that area I foresee difficulties.

The prospects for the development of social indicators seem better. It is at most a continuation of the direction in which many Federal statisticians and statistical series have been moving. Enthusiasm appears to be reasonably widespread in the Federal bureaucracy. President Johnson has indicated his interest. If development of series is pursued with prudence, their utility should become evident. The costs will be small in the totality of the Federal budget. State and local groups are expressing a similar interest in measures appropriate to their own locale. The fad for data banks pushes in the same direction. Business is becoming increasingly involved with information systems.

From the perspective of the first half of 1967, it appears that the development of more adequate social indicators may require that the Congress will have to make some budgetary decisions, but probably not decisions of great importance. The more important role for the Congress, it seems to me, will be that of remaining informed and influencing the direction of developments. Questions will arise. They will include such

issues as: the utility of various data series, the merits of larger samples so that measures can be broken down to State or local levels, who should pay for what, the right of people to refuse to give information, dangers to individual privacy, the uses to which information is put—its technical limitations, the inferences that are drawn.

Because of public controversies that have already arisen on many of these issues and which in turn have been reflected in the Congress, it would appear desirable that some committee of the Congress make it its function to monitor this development.

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## **CRITERIA FOR COMPANY INVESTMENT IN RESEARCH, WITH PARTICULAR REFERENCE TO THE CHEMICAL INDUSTRY**

*by* HAROLD GERSHINOWITZ

### **Introduction**

There are many ways in which a company can benefit from the doing of research. These include the intangible effect on public relations, a favorable reaction of stock market experts to the image of a scientifically oriented company, and the building up of the kind of good relations with the faculties of colleges and universities that aids in the recruiting of staff. However, the basic and primary reason that leads a company to spend on research money that would otherwise be returned to the owners as profit is the belief that spending on research will eventually increase the profits of the company.

A company can increase its profits by improving the efficiency of its existing operations or by making new investments that promise to yield a higher return on capital than would expanding, continuing, or even improving what is already being done. Research can help provide opportunities to do both of these. However, a company can also choose (or be required) to make investments in ways that do not require prior research. It is the responsibility of the board of directors to make the choices among the alternatives that will bring the greatest ultimate advantage to the shareholders.

The choices are not easy and the criteria are complex. Few companies have enough actual or potential resources to take advantage of all the opportunities available to them. Research in itself is not a profit making activity. Money must be invested and the other resources of the company must be marshaled in order to put the results of research to use. The amount of capital required varies from one industry to another. Historically in the chemical industry, three dollars of new capital has been required for each dollar spent on research (1). It would be senseless to do research if the results of research could not be put to use. Nevertheless, research must compete for company funds with the other activities that provide opportunities for investment. One example of alternative investment is the simple geographical expansion of the company's activities. A more complex example is given in the following paragraph.

In manufacturing industries, one of the dominant features of investment policy is the emphasis placed on the construction of facilities to pro-

duce new or improved materials. In extractive industries, however, a large part of the available capital must be spent to provide the means for exploitation of the mineral resources that have been discovered. In the petroleum industry, for example, the largest need for capital is for drilling wells and providing storage and transportation facilities in known oil-fields. It is the exploration activity, which includes the geological and geophysical prospecting, the acquisition of untested or unproved acreage, and the drilling of wildcat wells that provides most of the opportunities for investment of large amounts of capital. Thus, while exploration is very different from research, it does perform in the petroleum industry a function analogous to new-product research in the chemical industry. The chemical industry spends 4.4 percent of its net sales on research, the petroleum industry only 1.0 percent. Very few published data on exploration expenditures are available, but the Royal Dutch/Shell group of companies have given in their annual reports some figures for research *and* exploration, including dry holes, combined. For 1962 this sum was about \$225,000,000, nearly 4 percent of net sales, which is remarkably close to the average for the chemical industry.

Clearly, then, the balance between the alternative paths of investment varies greatly from one industry to another. The balance also varies from one company to another within a single industry, depending on the inclinations of the managers and the owners. For example, a railroad usually spends most of its effort and available capital on the improvement of its operations, whereas a pharmaceutical company usually builds plants to manufacture new kinds of medicines. But there are railroads that have chosen to go into trucking, or even into oil and chemicals (for example, the Chicago and North Western), while there are pharmaceutical companies that continue to make the same products year after year.

The amount of money a company spends on research usually depends on the relative importance it attaches to novelty in its operations or in the products that it manufactures. All the money may not be spent in its own laboratories or even on sponsored research in outside laboratories. Research of one's own is not the only way in which to improve or diversify. It is sometimes possible to purchase technology developed by others, by buying or licensing patents and know-how, or by the acquisition of a company that already possesses and is using the desired knowledge.

One must face the fact, since one's own efforts are only a small part of the total research effort of an industry, it is not at all likely that all the important innovations will come from one's own laboratories. A study of DuPont's activities between 1920 and 1950 has shown that of 25 major innovations in both processes and products only 10 were based on the inventions of DuPont's scientists and engineers (2).

Although one must always be on the alert for opportunities to purchase what others have done, such chances are not always available. When the

market is small or limited, it is possible for one company to satisfy all the demands and there is no incentive for it to license others. In many instances there is a substantial advantage to being the first to introduce a new process or a new product. Being first, however, involves considerable risks and costs, so that sometimes the innovator welcomes assistance. A strong patent position protects the innovator and makes it possible for him to get some return for his research expenses, even if he is not in a position to exploit the invention fully himself. The patent system is a strong incentive for doing research, and the possibility of obtaining a patent is often an important factor in the decision on whether or not to do a given piece of research. A strong patent position induces a company to buy technology from an inventor or another company.

A few companies in an industry can minimize their own research and depend on their ability to purchase technology. A whole industry cannot do this. Besides, there is evidence that the most successful and the most rapidly growing industries and companies are those that do the most research (3).

These complicating factors make it necessary to treat with caution the available statistical data on industrial research and to avoid sweeping generalizations. Nevertheless, there do seem to be certain patterns of behavior that characterize most industries and most companies within an industry. The report of the National Science Foundation, *Basic Research, Applied Research and Development in Industry, 1963* (NSF 66-15), contains data showing that the amount of money spent on research and development, as a percentage of net sales, varies from 0.4 percent for the food industry to 25.8 percent for the aircraft and missile industry. The average for all industry is 4.4 percent. These data are in Table A-34, p. 109 of the NSF report, which is reproduced here as Table 1. The 1965 figures for selected chemical companies, as given in their annual reports and other published data, are listed in table 2.

TABLE 1

TABLE A-34.—Funds for R. & D. performance as percent of net sales in R. & D.-performing manufacturing companies, by industry and size of company, 1957-63

Industry and size of company	1963	1962	1961	1960	1959	1958	1957
Total.....	4.4	4.3	4.3	4.2	3.9	3.8	3.4
<i>Distribution by Industry</i>							
Food and kindred products.....	.4	.3	.4	.4	.3	.3	.3
Textiles and apparel.....	.5	.5	.5	.6	.5	.3	(1)
Lumber, wood products, and furniture.....	.5	.5	.5	.6	.5	.4	(1)
Paper and allied products.....	.8	.7	.7	.7	.7	.7	.6

See footnotes at end of table.

TABLE A-34.—Funds for R. &amp; D. performance as percent of net sales in R. &amp; D.-performing manufacturing companies, by industry and size of company, 1957-63—Continued

Industry and size of company	1963	1962	1961	1960	1959	1958	1957
Chemicals and allied products. . . . .	4.4	4.3	4.4	4.5	3.9	3.8	3.5
Industrial chemicals. . . . .	5.3	5.1	5.3	5.8	4.9	5.4	5.0
Drugs and medicines. . . . .	4.4	4.3	4.3	4.6	4.3	4.1	3.6
Other chemicals. . . . .	2.7	3.0	2.8	2.3	1.99	1.5	1.3
Petroleum refining and extraction. . . . .	1.0	1.0	1.0	1.1	1.0	1.1	.7
Rubber products. . . . .	2.2	2.1	2.2	2.0	2.0	1.8	1.7
Stone, clay, and glass products. . . . .	1.8	1.7	1.6	1.7	(2)	(2)	(2)
Primary metals. . . . .	.8	.8	.8	.8	.6	.7	.5
Primary ferrous products. . . . .	.7	.6	.7	.6	.5	.6	(1)
Nonferrous and other metal products. . . . .	1.0	1.1	1.2	1.1	.9	.7	(1)
Fabricated metal products. . . . .	1.6	1.5	1.4	1.4	1.4	1.7	1.6
Machinery. . . . .	4.0	4.0	4.1	4.6	4.3	3.8	3.4
Electrical equipment and communication. . . . .	10.0	9.4	9.8	10.7	10.7	10.3	7.6
Communication equipment and electronic components. . . . .	12.5	12.1	12.3	12.9	12.4	11.3	(1)
Other electrical equipment. . . . .	7.5	7.4	8.1	9.2	9.5	9.7	(1)
Motor vehicles and other transportation equipment. . . . .	3.4	3.5	3.9	3.0	3.3	4.2	2.9
Aircraft and missiles. . . . .	25.8	23.0	23.1	22.9	20.6	17.7	16.8
Professional and scientific instruments. . . . .	8.4	8.1	7.2	7.6	7.9	7.8	7.0
Scientific and mechanical measuring instruments. . . . .	9.8	9.4	8.5	10.8	11.0	10.2	9.5
Optical, surgical, photographic, and other instruments. . . . .	7.4	7.2	6.3	5.6	5.8	6.3	5.2
Other industries. . . . .	.9	.9	1.5	1.3	.8	1.3	(1)
<i>Distribution by Size of Company (based on number of employees)</i>							
Less than 1,000. . . . .	1.9	1.8	1.8	1.6	1.7	1.3	1.8
1,000 to 4,999. . . . .	2.3	2.2	2.2	2.2	1.8	1.8	* 1.8
5,000 or more. . . . .	5.2	5.0	5.2	5.1	4.8	4.8	* 3.9

<sup>1</sup> Not separately available but included in total.<sup>2</sup> Ratio is reflected in the "Other industries" group.<sup>3</sup> Estimated by the National Science Foundation.

TABLE 2.—Selected chemical companies research as percentage of net sales, 1965

<i>Company</i>	<i>Percent</i>	<i>Company</i>	<i>Percent</i>
Allied	3.0	General Aniline	4.0
American Cyanamid	4.8	Hercules	3.4
American Enka	2.6	Hooker	3.3
Atlas	5.1	Monsanto	4.8
Celanese	1.7	Stauffer	2.5
Diamond Alkali	3.2	Union Carbide	3.8
Du Pont	3.7		

The chemical industry provides many specific examples of the importance and the relevance of the factors discussed in the preceding pages. The next part of this essay will deal with that industry.

### The Chemical Industry

Although the chemical industry is one of great diversity, with products ranging from basic chemicals such as caustic soda and sulfuric acid to the complex molecules of the antibiotics, it has a remarkable homogeneity, probably based on the fact that the starting materials are chemicals and the end products are still just chemicals. An automobile, a television set, or a refrigerator are things quite different from their components or starting materials. A plastic, a solvent, or a synthetic rubber, although much changed from the original starting materials, are still chemicals. A large part of the output of the chemical industry is sold to other chemical companies to be used as starting materials for more complex chemicals. The relationship between the starting material and the product is obvious and the language of chemistry permeates the entire company, from the research laboratory to the sales office. The relationship between what goes on in the research laboratory and the normal business of the rest of the company is clear. This probably accounts for the fact that the chemical industry was among the first to set up its own research laboratories. Many chemical companies can point with pride to the fact that a large percentage of their current production consists of compounds that did not exist 20 years ago.

The recognition and the acceptance of the importance of research in the chemical industry does not automatically solve all the problems of doing research. One will not find unanimity in the opinions or theories about the ways in which to decide how much and what research to do. At one extreme are those who believe that research should be the servant or tool of the businessman and should work on those problems that the businessman considers to be the most important, and at the other extreme are those who believe that the scientist should be left completely free and that it is the responsibility of the businessman to find some way of using the discoveries of the scientist. These opinions are to a large extent vestiges of traditional practices in both research and business management. Modern practice seeks a middle ground.

The amount of research being done in the laboratories of the chemical industry has been increasing rapidly for more than 20 years. The increase has been both relative, in the sense that research has been an ever increasing percentage of the total investment of each company, and absolute, in terms of the number of scientists and engineers engaged in research and development. This growth has had two important consequences. In the first place, the fact that research is now such a large, and identifiable, fraction of the outlay of a company makes it necessary for boards of directors to pay attention to it. Second, the larger total amount of research being done makes it more and more difficult for any one company to gain a significant technological lead over other companies in the same field. The major problems and opportunities of the industry are usually quite obvious to the initiates, and consequently many laboratories work on similar problems at the same time. Even when one company has made a significant innovation, competitors can usually catch up very quickly.

Both of these consequences of the growth of research have made it obvious that an integration of research into the complete structure of the company organization is desirable. An innovation that is technically sound can be economically unsound if the costs of the research and the new manufacturing facilities cannot be recovered before the activities of competitors make the innovation obsolete or reduce its profitability by over-saturating the market. This means that in the later stages of research there must be close agreement between research on the one hand and marketing and manufacturing on the other on the value of an innovation and on the optimum timing for its introduction. The research man must keep in touch with competitive activity in his field. Patents must be applied for quickly in order to forestall competitors.

An invention, or the discovery of a chemical reaction, is usually very far from a feasible process or a marketable product. Research as challenging as the original project may be required in metallurgy and in chemical engineering. New problems in chemistry may arise during the transition from the laboratory to the marketplace. Both the original research workers and the ultimate appliers of the research must take part in a continuing exchange of knowledge and information until production has become routine. There is no longer room for the isolationist at either end.

In response to these needs and requirements there is emerging a fairly general pattern of organization and of division of responsibility that attempts to tie research objectives to those of the company while allowing the freedom of activity that is necessary if research is to come up with those new, and usually unanticipated discoveries that provide the opportunities for major profitable investments.



In this organization, research (and usually development) is set up as a division or department, equivalent to manufacturing and marketing. Mechanisms are then established to make possible the necessary interactions of these three functions with each other, and for their combined interaction with finance and overall company planning. The exact position and function of research within a corporate hierarchy depends on the detailed structure of the corporation. Most large companies differ from others in both the geographic and technical scope of their activities. Accordingly they have different ways of assigning geographic and functional responsibilities. One can find all conceivable variations of centralization and decentralization of both geographic and functional responsibilities. Indeed it sometimes seems that there is a continuous pendulum-like swing between decentralization and centralization in every large company. Several descriptions of current types of organization can be found in recent publications (4). In all modern organizations, however, research has that position in the structure that makes possible the interactions described in the preceding paragraphs. This fact of corporate organization is implicit in the discussion of criteria which follows.

A major problem in fitting research into corporate planning arises from the most important characteristic of research, whether basic or applied, which is that its results cannot be anticipated. Although all forecasting involves uncertainties, the uncertainties in predicting the consequences of research are different and greater, both qualitatively and quantitatively, from the other kinds of prediction used in business planning.

For a laboratory with some past record of achievement, one can estimate what percentage of the work being done is likely to have some success. It is possible to estimate the commercial value of a project if it were to be successful. One can also estimate the probable duration and cost of a project. All estimates, whether commercial or technical, however, will have to be changed as the work progresses. The potential market or the value of the product may be changed because of the activities of competitors. The estimates of the probability of the success and the cost of the research will change almost continuously as new results, favorable or unfavorable, are obtained. Unexpected research results may make desirable a complete reevaluation of original objectives or offer the possibility of new objectives. All these factors call for a continuing review of the progress of research and of the company's desires and objectives.

The medium for making this possible is the research program. A typical research program lists in some detail the nature of each research project and its probable cost for the coming year in terms of both money and manpower. If a project is in an advanced stage of research or

development an estimate of the time of completion and total cost of the project is given. This program is the result of a continuous interaction between the representatives of the research function on the one hand and those of manufacturing, marketing, and other interested elements of the company on the other. Any research laboratory worth its salt has more ideas than it can undertake to work on with the resources available. The program represents a compromise allocation of the resources to those projects for which the probability of success and the value of successful results seem the highest.

Such a program must be approved on several levels. The scientist or engineer directly concerned must vouch for the technical feasibility and for the manpower estimates. His superior in the research organization must vouch for these also and, in addition, for the availability of the needed resources and manpower. The head of the research organization must make sure that the totality of the program is within the means available to him or requested by him. The manufacturing and marketing departments must concur with the proposed program, confirming that they are prepared and desire to put into effect the results of the research, always provided that the time and cost estimates can be adhered to. These are not *ex post facto* approvals. Each involves, as I have said above, a continuing dialogue between the parties concerned. In some companies they are required to confirm that the costs of the research are acceptable to them.

The final review is by general management and the board of directors. These must confirm that the objectives of the research are compatible with the objectives of the company, and that the financial support for applying the research can be made available. When, as sometimes happens, the research program includes projects that are outside the scope or charter of the existing parts of the company, they must be prepared to create the means for applying the research.

The criteria for decision vary according to the nature of the research project. There is, and will be, a great deal of discussion about the difference between basic and applied research. A more useful way of classifying the research done by industry is in terms of the objectives for which the work is being done. For the chemical industry the following classification can be used.

1. *Science-oriented research.* This is research done in order to increase the amount of knowledge about a field of science, a phenomenon, or a chemical compound. Although it is usually in subjects related to current or potential commercial activity of a company, it is not directed toward any specific new process or product. This kind of research is indistinguishable from basic research done in universities.

2. *Process-oriented research.* The objective is to decrease the cost and improve the efficiency of making a known product from an available raw

material. It may, and often does, involve novel chemistry and novel technology.

3. *Raw-material-oriented research.* The objective is to increase the value of products or raw materials available to the company by transforming them into new and more valuable products.

4. *Product-oriented research.* The objective is to find alternate and wider uses for existing products and to find new products that will be better than those available for existing applications.

5. *Customer-oriented research.* The objective is to find products to add to those already sold to existing customers.

The last four can be grouped together as "business-oriented" research. For all of these, development is a part of, and inseparable from, the research program.

The criteria for deciding whether or not to start or continue a research project are different for each of the five classes.

Projects for science-oriented research almost always originate within the research organization. The exceptions usually come from a decision of the management to extend the field of interest of the company in new directions. For example, a company that has not previously made or sold pharmaceuticals decides to look for opportunities in this field. The research function may then be asked to build up a competence in biochemistry. Even in such cases, however, the actual proposals for projects will come from the research workers. The director of research usually has at his disposal a sum of money that can be allocated to science-oriented research. This sum will vary greatly from one company to another. Some figures can be found in the data reported by the National Science Foundation (where this kind of research is called "basic"). For companies engaged in the manufacture of industrial chemicals the industry average is 11 percent of the total amount spent on research and development. For firms making pharmaceuticals and medicines the average is 15 percent (NSF 66-15, Table A-65). This is much higher than that for any other industry except petroleum refining, which is really a form of chemical industry restricted to one class of compounds, hydrocarbons. The average for all industry is only 4 percent.

Chemistry is a very broad and rapidly moving field of science. The industry cannot rely on the universities for either the amount or the variety of basic knowledge needed to provide background and starting points for its applied research. The amount of science-oriented research done by any one company depends on the distance of its activities from the frontiers of chemical knowledge. Those closest to the unknown do the most. The evaluation of the need for science-oriented research is intuitive rather than quantitative. Probably the most important criterion used by the director of research is his evaluation of the competence of the scientist who makes the proposal. The next criterion is the relevance to the overall

program of the laboratory. In some cases the relevance may seem remote but the decision of the director of research on an individual project is rarely reviewed by higher management, although he may be called upon to justify the total amount he is using for science-oriented research. Just as the director of research bases his judgments on his evaluation of his research staff, top management relies heavily on its confidence in its research management.

Economic factors weigh more heavily in decisions about the four kinds of business-oriented research. In the first place, there are usually more alternatives on which work may be done. In the second place, while the scale of science-oriented research projects remains fairly constant, the other kinds will usually need larger-scale effort as the work progresses, both to bring the process from laboratory glassware to the materials of construction of large plants and to prepare samples for testing and evaluation. There are, therefore, two important decision points for business-oriented research; first, whether or not to start, and then whether to continue into the more expensive development or product-application work. At the second decision point, patent considerations become important.

The criteria for the initiation of a project are not much different from those used for science-oriented research. Although the incentive for thinking about possible projects will often come from the manufacturing or marketing functions, the specific proposals for individual research projects will usually originate with members of the research staff. The major point of difference from science-oriented research at this stage is that the marketing and manufacturing functions will have a substantial voice in deciding how much money or effort to allocate to each of the potential projects of interest to them. Their decisions are based primarily on their estimation of the importance to them of a successful result.

After an initial scientific success has been made, economic criteria become more and more important in subsequent decisions. In process-oriented research it is possible to estimate with good accuracy the cost of carrying the project through the stage of commercial plant design and the time needed to complete the project. It is possible to estimate with fair to good accuracy the capital and operating costs of the new process. For this kind of research one can therefore make an early estimate of the profitability of the project and compare it with other possible courses of action. This very fact introduces a danger. Because of this suitability for quantitative evaluation, executives not trained in research or familiar with the ways of research often tend to favor such projects over more speculative ones, even though the latter, if successful, might promise a much greater profit to the company (5). It is one of the responsibilities of the research director to make sure that during the formulation and approval of the research program a proper balance is achieved be-

tween kinds of research for which costs and profitability can be calculated and those less susceptible to quantitative evaluation.

The costs and consequences of raw material-oriented research are less easy to predict than those of process-oriented research. Even though one may start from a base close to the company's experience and activities, the eventual product may be one never before made commercially by anyone. In that case an expensive period of market development is necessary in order to find out what the product can be used for, what it would be worth to the user, and how big the market would be (6). Close cooperation between marketing and research is necessary at this stage. If it appears that the product will be used in food, medicine, or pesticides, an extensive, prolonged, and expensive series of studies of toxicity must be started. Large quantities of the new material must be made available. Even when the product is an old, well-known chemical, if the new process is much different from those already in use, particularly if it starts from a new raw material, there must be extensive testing for and by customers, in order to make sure that traces of impurities, almost always present in commercial chemicals, will not be deleterious. This is particularly important for food additives, medicines, and pesticides, for which the whole gamut of toxicity tests must usually be repeated.

It sometimes turns out that a process that is demonstrably more efficient and produces a superior product is not worth adopting because the advantages are not convertible into enough dollars to pay for the new equipment and for the cost of proving the suitability of the new product for the market. The lack of realization of this fact of economics contributes to the belief that industry scraps or conceals new inventions in order to protect existing investments. Even the research staff can misunderstand and resent the non-use of their inventions. New technology must always compete with old technology economically as well as scientifically. The ramifications and costs of a new process can extend far beyond the limits of the equipment in which the process takes place, ranging from new waste-disposal problems to the need to convince the customer of the need for and desirability of modifying his equipment or his methods of use of the product. The participation of research in the total decision-making process is essential if the research staff is to understand that non-scientific criteria can often be decisive.

Proposals and even projects for product-oriented and customer-oriented research often originate outside the research organization. A salesman or a customer will point out weaknesses in existing products and explain what kinds of improvements would be desirable. A marketing manager will discover that his salesmen are making many calls on customers to whom they can offer only one or two products and will ask whether research (and manufacturing) could supply his staff with other products that would be useful to these customers.

Although both financial and scientific criteria are important for deciding on the program for these two kinds of research, the scientific criteria are often the more important. Many of the proposals fall into the category "it would be wonderful if," and it is the duty of the research man, often painful, to point out that it would indeed be wonderful but that it contradicts the laws of science, or that it would cost so much or take so long to find out that the customer would no longer be interested. The research man may even be forced to agree that it would be wonderful and might be feasible but he doesn't have any ideas on how to do it.

Financial criteria are also important, however, and often the department in which the proposal originates is asked to estimate the value to them of a successful result. In order to allow for unsuccessful projects, the estimated value must be several times the estimated cost of the project. Sometimes, however, the need to retain the good will of an important customer, or similar intangible considerations, will lead to the acceptance of a project that would be rejected on the basis of purely financial criteria. Thus, for customer-oriented and product-oriented research, the judgment of the marketing executive plays a role similar to that of the research executive in science-oriented research.

### Some Remarks on Government Research

Analogies are always dangerous, but they are often useful. How can one translate the practices of industry into those of Government? How much of the experience of industry is relevant to Government? Much has been made in this analysis of the importance of profitability as a criterion for making research investments. In defense or in public welfare other kinds of criteria are dominating. Yet financial criteria are, in another sense, important for the Government as well. There is a limit to both financial resources and technical manpower. Other papers in this report are proposing and describing potential opportunities for applied research by the Government. Choices must be made. The social value of a solution is frequently cited as the justification for research.

Yet there are dangers in using social values as criteria. One lies in the fact that most of the problems on which the Government should be doing applied research are of tremendous importance. As described in this report, the research program of an industrial company consists of proposals by the research staff to do specific things. Approval of the program may depend on an evaluation or estimate of the importance or profitability of a solution, but rarely is a project taken on just because there is a big problem to be solved. The *sine qua non* of research is an idea of how to proceed to a solution. One of the more difficult tasks of a research director is justifying to other executives his failure to attempt to solve some of the problems that are of overwhelming im-

portance to them. Sometimes pressures cannot be withstood. Enormous amounts of money have been spent, fruitlessly, on two of the most important problems of the petroleum industry—utilization of natural gas in remote parts of the world, and desulfurization of crude oil or heavy residues. Perhaps the same ground rules do not hold for Federal research, because, for industry, the adjective "economical" should be understood before utilization and desulfurization in the preceding sentence. Nevertheless it is true that the two examples cited are cases in which research was demanded by harried executives and research men yielded to constant pressure.

The recognition and identification of problems, the decisions that they should be attacked with the aid of Government funds or facilities, the setting of priorities, all these are the proper responsibilities of the people and their representatives. However, the allocation of large amounts of money should follow—not anticipate—the research proposals. The actual research program, including estimates of how much effort it is wise to assign to a project, should originate with the research workers, with due allowance being made by them for the social importance of the problems waiting to be solved. Only the mediocre and those with few original ideas will be tempted by easy access to funds that have been allocated in the mistaken idea that science can solve any problem, if only enough money is made available. But it is certainly true that in many cases, for example, in some of those cited in Dr. Weinberg's paper, promising solutions seem to be at hand and only a vast deployment of resources is necessary.

There seem to be many parallels between industrial and Federal research. The distinction made in this essay between science-oriented and business-oriented research is obviously the same as that between basic and mission-oriented research. The subdivision of business-oriented research are not so easily translatable, but the ways in which the mechanisms of decision-making are related to the origins and end uses of the research projects should have some relevance. Applied research should not be carried out in isolation or done in the abstract. It succeeds best in an environment in which the use or potential use is always visible and in which the potential user is a part of the team in a way that stimulates him to accept the new. People do not take kindly to solutions to their problems that are thrust on them from the outside. If they feel that they have contributed to the definition of the problem and that they have the opportunity to take part in the progress of the work, they are much more likely to try the proposed solutions. In addition, the intelligent cooperation of the man familiar with actual field conditions will keep research workers from going up blind alleys or producing inoperable solutions.

In industry it has been found that one of the most important requirements for bringing the results of research to practical application is some means for keeping research and its users in constant communication. Most large companies, although their laboratories are usually remote from the central office, do have in that office some sort of a research, or research and development department. The principal function of this department is not to direct the work of the research laboratories but to make sure that research advice, information, and participation, are available during the daily processes of decision-making. In cases in which the research work is very different from the normal business activity of the company, this department acts as an impedance-matching device. If the Government is to embark on an expanded program of applied research in fields other than the military, it would seem essential that appropriate links with research be set up in each department or agency concerned.

There seem to be less pressing needs in Government for some of the other features of the industrial research planning and decision-making procedures. The element of competitive pressure is non-existent (except in military research, of course). There is not so much pressure or need to put into practice the first results of research. If a more promising solution is on the horizon, it may be better to wait. (Consider, for example, the current controversy about the Salk and Sabin polio vaccines.)

Whether there should be a single director of research for the Federal Government's total activity is very debatable. There seems little doubt, however, that each department or agency that does or wishes to carry out applied research should have a director of research. Just as in industry research is integrated into the organizational fabric of the company, so in Government it must have a position in which it can become informed of the nature and magnitude of the problems susceptible to technological solution and participate in the planning process.

### Summary

Although the chemical industry has some problems and procedures that are unique to it, most of the following is applicable to all industry.

Research is an activity of a company that makes it possible for the company to increase its profits. Research must compete for company funds with other activities that provide opportunities for increasing profits, such as the purchase of other people's research or growth in ways that do not require research. Research in itself cannot make any profits for the company. The results of research must be put to use by the manufacturing and marketing departments of the company, and the funds that make this possible must be supplied by those responsible for policies and financial resources as a whole. The organization of the



modern company, and the place of research within it, must facilitate the cooperation needed if research is to be responsive to the actual needs of the company and if those who must apply the results of research are to understand and approve the research program. Since some of the results of research cannot be anticipated and may lead a company away from current lines of activity, the ultimate decisions regarding the scope of the research program are made by authorities that are responsible for the overall welfare of the company—the president, the executive committee, or the board of directors.

The criteria for deciding on research projects usually include both scientific and commercial elements. In order to achieve a balanced and feasible program, those in positions to make contributions to these decisions take part in the deliberations and share the responsibility for the recommendations. Although research is almost always a department or division reporting to the chief executive, those in charge of manufacturing and marketing have much of the responsibility for those parts of the research program that are intended to aid their parts of the operations of the company. At the same time, the research executive has the responsibility for ensuring that the research can be done in the required way, and for maintaining a balance between short-range, easily envisaged projects amenable to profitability analyses, and the more visionary projects that might provide exciting and, in the long run, more profitable ventures for the company.

While the objectives and criteria for Government-sponsored research are often different from those of industry, many aspects of the organizational structure and decision-making procedures of industrial research are applicable to Government.

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## THE TRANSITION FROM RESEARCH TO USEFUL PRODUCTS IN AGRICULTURE

by STERLING B. HENDRICKS

Agriculture is one of the largest industries. The annual value of product at the farm gate is \$41 billion from 3.7 million farms. The success of American agriculture is a model for the world and a bulwark for the Nation's needs. This enviable position has not come by chance, but instead is deeply rooted in the pattern of American life. It is sustained by efficient management and protected by effective research.

The large number of farmers and the pattern of agricultural development leads to a unique interplay between public and commercial research. I present two examples of this pattern before taking a more general look at agricultural research. The examples are the tomato and the chicken.

Tomato production in the United States was 5.5 million tons in 1965, with a value of about \$350 million. A half-million acres were planted. Research and development costs were less than \$6 million with about equal division between industry and the public.

Labor is a major problem in tomato production. It is closely followed by ravages of diseases and attacks of pests. The fruit is highly perishable and the consumer is sensitive to quality in both the fresh and the processed fruit. Research and development to meet the needs deals with mechanization and breeding. The mechanical part of the job is both industrial and public, but breeding is mainly public.

Tomatoes are a long-season crop, slow in early development. Young plants, which must be carefully guarded against disease, are grown in warm areas from which they are shipped to production regions. Machines are used in transplanting, but these require riders to place each plant. Improvement is needed. The next bottleneck is harvest, which has been a job for stoop-labor. Changing laws with respect to admission of foreign laborers forced the issue of mechanization. Can an acceptable system be developed? "System" implies demands on both the machine and the plant. The machine now used harvests the entire plant which, for many varieties, sprawls over the ground. It then shakes off the fruit, separates debris, and transfers the fruit to a container in an accompanying truck. All of this must be done with a minimum of injury. Several models of machines have been developed to harvest tomatoes for processing. To obtain the desired plant is more difficult. Most of the fruit must be ripe at harvest and the growth should be upright. To achieve the first objective

the first fruit to ripen must stay on the plant a long time without spoiling. This requirement and the need for upright growth can be met only by breeding. The system combination is reasonably successful and mechanical harvesting is now possible. Economic adjustments, however, are also involved. R. L. Gibson, Jr., President of the food firm, Libby, McNeil, and Libby, recently said, "mechanization initially raises cost." The tomato industry "has gone into mechanization too rapidly." W. B. Murphy, President of the Campbell Soup Company, said, "Possibly there will be a combination of hand-harvesting for the first ripening of the fruit plus mechanical harvesting for the balance."

The plant breeder is really the man behind the tomato, and most other crops. Behind him is the geneticist. The breeder is chiefly, but not entirely, a public employee. His objectives include disease control, yield increase, and quality improvement. A plant lacking resistance to certain soilborne fungi will often die before fruiting. A first step in breeding is to acquire the known germ plasm of the species in question and related ones. Then the genetic features of each species are studied, a process that requires decades to build up a background of information. The first tomato species resistant to fungal attacks were introduced more than 40 years ago. They have been greatly improved since then and the degree of resistance to several of the common soilborne fungi has been increased. Resistance to fungi invading the foliage is more difficult to attain. Resistance to insects is also desired and there are indications that it might be realized. This is particularly true for aphids and mites, which transmit virus diseases.

The disease-resistant and quality features have to be combined genetically with those required for mechanical harvesting. Here, the breeder draws on his stock of genetic strains. The final fruit must be of suitable quality, deeply colored, with a high content of solids and plant acids. Some fruit properties are best displayed by hybrids, which result from crossing of two strains. Production of hybrids for the tomato is now accomplished by hand emasculation and pollination. Field production of hybrids through use of male sterile lines is approachable and is being developed at the University of California, Davis. But the breeder's task, at the best, is a slow and tedious one, in which the time unit is nearer to a decade than to a year.

The chicken, no less than the tomato, requires a systems approach. The poultry industry in the United States is now highly specialized in four areas, for producing the starting chicks or poults, eggs, broiler-type chickens, and meat-type turkeys. In 1945, only 345 million broilers were produced. The increase was sevenfold by 1965, while the time to reach marketable weight of a bird was reduced by a third. The value of broilers produced at the Nation's farms is now about \$3.5 billion.

Science, as well as management, underlies the dramatic change. The poultry industry depends more on science to solve breeding, nutrition, disease, and economic problems than does any other agricultural enterprise. The bird is tailored for the job. Changes in the type of bird in the United States and Canada are startling and rapid.

Egg production is one segment of the industry. It is based on selection of genetic lines for high egg yield of high quality. Breeders using methods of population analysis place maximum selection pressure on the age at sexual maturity. The rate of egg production, which involves the duration of a clutch and absence of broodiness, is also emphasized.

Producers of broiler or meat-type chickens emphasize selection for body weight, rapidity of growth, and meat quality. There is a high correlation between mature body size, rate of growth, and efficiency of feed use. The broad-breasted turkey is well known to consumers. It has been an important achievement in turkey breeding.

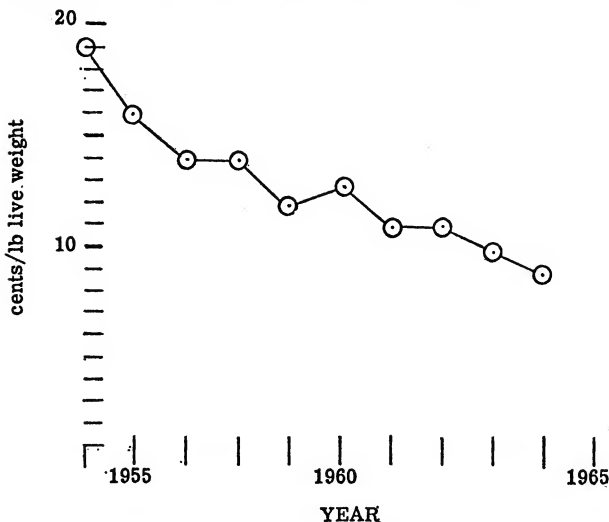
The breeder, who was once a poultryman with a good eye for selecting superior birds, has been replaced by a Ph. D. geneticist with access to breeding stocks and with a computer at his elbow for solving problems of population genetics. Improved hybrid stocks are available for crossing in chick production. The end is not in sight for breeding. Egg production and reproductive rate of meat-type birds leave much to be desired, as does the viability of all poultry types.

The potential of the highly developed bird has to be realized through correct feeding and disease control. The food required for weight gain of one pound for a young bird has been reduced by 45 percent in 20 years. Major changes rested on the discovery of vitamin B-12, which was noted about 30 years ago as a factor in chicken manure essential for chick growth, and the advent of high-energy foods in the early 1950's. We know more about the protein and amino acid requirements and the energy, vitamin, and mineral requirements of the growing chicken than for any other animal, including man.

Successful poultry raising is a monoculture involving crowding, and thus with the ever-present threat of epidemic disease. Half the flock can be lost in a week. Disease control is obtained chiefly through sanitation and medication rather than by breeding, as for the tomato. A most significant step was the discovery of coccidiostats such as sulfamethazine. Vaccines that can be supplied in drinking water were developed for Newcastle's disease and infectious bronchitis. Antibiotics were developed for bacterial diseases accompanying crowding. Each success, by creating a receptive industry, reduced the time from initial research to wide usage. The period now can be as short as five years, but increasing questions of interplay between poultry medication—use of stilbesterol, a hormonally active compound known to have some cancer-producing activity for

weight gain, is an example—and human consumption will probably increase this time.

Each introduction of a new factor involves a new management system. Cost of labor again is limiting. Structures must house large numbers of birds with special arrangements for watering and feeding. The competitive pressures for improved production is great in various sections of the country. It is reflected in the changes in the selling price of chickens shown in the graph on the facing page. There is an element of regret here, however, for the technical advance has made chicken production economically hazardous. Production tends to move to sections of the Nation where wages are low, with accompanying lack of social well-being.



With the examples of the tomato and chicken in mind, I now turn to more general matters. A new discovery like vitamin B-12—the isolation coming from research on wool and lamb production in Australia—or a new tomato variety, can greatly change production methods. But there are certain other factors in a successful agriculture that are non-technical and should not be forgotten. In the United States, these include a temperate climate, ample water, fertile soil, and abundant land, as judged by the standards of other countries. Agriculture has prospered here in a

favorable pattern of education, rural development, reasonable land holding, credit policies, and economic controls, as well as biological, technological, and marketing developments. Underdeveloped countries often lack these factors, and thus are limited in their possible gain to what is achievable through use of biological and technological developments alone.

Objectives of agricultural research are in part the same as those of research in industry. These are to introduce new methods and products, improve products, reduce costs, guard against diseases or adverse factors, and increase output per unit of labor. Land and other resources, which are the very means of production, can be lost, however, so they must be used with an awareness of conservation. Water cannot be wasted nor salts allowed to accumulate. Pollution must be guarded against, whether coming from neighboring regions and cities or from agriculture itself.

Agriculture is sometimes said to resist innovation. Perhaps this is true if one thinks of farming as an old art. Perhaps it is true because existing approaches such as breeding still have so much to offer. Or possibly it is true because of lack of security of a small producer in accepting risk. New products and new methods, however, are introduced as rapidly as in most basic industry. Farming has possibly changed more rapidly than the metal industries. Most wheat and corn varieties now grown were developed in the last 20 years. Safflower production for edible oil use increased two and a half-fold in ten years. The total number of cattle bred by artificial insemination has increased threefold since 1950, and egg implantation as well as sex determination is likely to be realized. Dried-egg production increased two and a half-fold in ten years almost solely because of product improvement. Urea has been introduced as a source of nitrogen in cattle feed to replace a part of the protein intake. Frozen foods in ever-increasing amounts and gain in quality have chiefly been introduced since 1945 because of process improvement. These and many other technology-based advances are an essential part of a changing agriculture.

Some historical aspects of agricultural research should be noted here. A need for farm research and the supply of suitable breeding stocks and seeds arose by 1850. It also involved the use of fertilizers. The farmer needed more knowledge and the aid of trained men. The happy solution was in the creation of land-grant colleges in each state with associated experiment stations and a national Department of Agriculture. The establishment and public policy thus were for a large measure of research and development through public support. Objectives of the nation as a whole as well as those of individual farmers were also served by this approach, particularly where desirable practice involved conservation and long-range development instead of immediate return to the farmer.

The State Agricultural Experiment Stations are the most effective links in bringing new products, discoveries, and advances in knowledge into farm practice. They serve to bring both commercial and public findings into use. They also transmit the needs of the farms to the research workers. Each ensures maximum development within a state while the national Department of Agriculture integrates the whole on a regional and eventually a national basis.

The Experiment Stations are the models for a first step forward in the least-developed nations. Details of their operations offer much even to some advanced countries. One of the struggles of Soviet agriculture has been to understand the operation of this system and to sense how it might be brought into use in the Soviet Union. Pakistan and India see in the system a step toward answering their difficult questions of food production.

Agriculturally related industrial research and development has existed a long time. It is rapidly increasing and now constitutes about half of the \$850 million total of agricultural research for the United States. Milling, ginning, packing, brewing, food, feed, sugar, canning, farm machinery, fertilizer, pesticide and herbicide production, leather, and other industries are examples. Interests of these industries and public research mesh together quite well. Both might work together on the same machine, the tomato harvester, for instance, or on a similar pesticide. Sometimes, though, the public interest clashes with the industrial development. An illustration is the public maintenance of competition between cotton and industrially developed fibers in tire cord and clothing. The public research is not directed to maintaining the present materials to the detriment of their competitors, but rather to maintaining a competitive position for them in an advancing technology.

The public policy is not only to fill a broad function, but also to ensure development even when early profit is minimal. Development such as hybrid seed and chick production always passes on to industry, when adequate profit and satisfactory operation become evident. There are frictions, particularly about patents and more often about claims. Patents are viewed by industry as a protection for development costs and a reward for ingenuity. Public development assumes the burden of cost. Reward for ingenuity remains.

Regulation can lead to friction between the Government and commerce. This is particularly true if all features of the regulation have not been publicly presented. A difficulty with regulation is that it is far easier to set an unreasonably low-tolerance limit for some adverse factor than it is to establish the safe limit. Industry would be satisfied with the safe limit, but sometimes considers itself put upon by an unreasonably esti-

mated tolerance. An example in this area is the question of the suitability of solvent-extracted whole-fish meals for human use, which, though perfectly satisfactory, have long been delayed in use. Meat inspection, which has been unpopular at times, is an example of a satisfactory regulation.

Regulation also involves quarantine. Foot-and-mouth disease, a highly contagious virus infection of cloven-hoof animals, is endemic in many parts of the world. It is an ever-potential threat poised toward the animal industry of the United States. When it does invade, as it last did in 1953, containment and slaughter are required for control. It is better stopped at the borders, which sometimes means embargo on imports. A similar situation exists with respect to several fruit flies. The Mediterranean fruit fly was found in Florida in 1929, but was contained by drastic quarantine. The oriental fruit fly was found in Hawaii in 1946, following the heavy war traffic with the Orient, and rapidly multiplied to be a major pest. The Japanese beetle came to the United States before the necessity for control was appreciated. In a sense, American agriculture must be prepared both to live with the rest of the world and still to protect itself. Quarantining is preventive even as much successful medicine is preventive. To be effective, however, it requires deep knowledge of both the objective organism and the transmitting agent.

Mission-orientation of research is the greatly predominant rule in both agriculture and industry. This is sometimes decried from an academic point of view. My experience is that a mission-oriented approach enhances rather than handicaps advance in knowledge of principles essential to a mission. In short, missions are satisfactory provided they are not trivial. An example is a mission to enhance the yield of soybeans in mid-continent America. Working toward achievement of this mission—perhaps a twofold increase in yields—probably provides a greater chance of finding the controls of flower differentiation than does unrestricted research.

The economic magnitudes in agriculture are immense. Some values from "Agriculture Statistics 1965" are found on the facing page. Fortunately, the whole of this great mass can be segmented for both production and supporting research. The problems of agriculture on which science bears are, for convenience, mechanical, processing, chemical, and biological. Economics and production knowledge, of course, require all the modern capacities of computers, which are developed through science.

The mechanical side of farming includes motive power (tractors and trucks), plows, planters, cultivators, sprayers, harvesters, and dryers. Specialized storage structures such as silos, barns, and cribs are needed.

Dairying requirements are so specialized that they require separate attention. Irrigation devices include pipes, pumps, sprinklers, and automatic controls. The investment exceeds one year's value of the product.



Product	Production (billions of units)	Farm value (billions)	Acres (millions)
Beef and veal.....	20.....	\$6.0.....	
Pork.....	12.5 pounds.....	1.9.....	
Lamb.....	0.7 pounds.....	.2.....	
Chickens.....	8.7 pounds.....	1.2.....	
Eggs.....	65 eggs.....	1.8.....	
Milk.....	126 pounds.....	5.3.....	
Corn.....	3 bushels.....	4.0.....	57.0
Wheat.....	1.3 bushels.....	1.8.....	50.0
Cotton.....	7.5 pounds.....	2.3.....	14.0
Soybeans.....	0.7 bushel.....	1.9.....	30.0
Sugar beets.....	.....	.3.....	1.5

Machines are often designed for a single use, as is the tomato harvester. Machines for harvesting hay, sorghum, cotton, corn, or sugar cane could serve equally well as examples. They have the common feature of greatly saving labor at some detriment to quality or amount of product. They are very ingenious in design. Development time for a fully acceptable machine is often of the order of 10 to 20 years, as it was for the cotton and corn pickers. Improvement continues, as it has for the automobile. Modifications are also required in the machine to adapt to practices that are themselves modified. Examples are defoliation of cotton before picking and breeding of dwarf sorghums to facilitate use of combines. Changing of practices and machines to allow cultivation of four rows instead of two immediately requires a change in farm systems management.

Development of the internal-combustion-engine tractor led to mechanization in farming. About 5 million tractors are now used in the United States. The horse and the mule were replaced by them in one generation between 1920 and 1940. Upwards of 75 million acres of land were released that had been used to support draft animals. This acreage in part facilitated increase in meat production to match the changing food patterns of the people. Tractor development is almost entirely done by farm machinery and automotive companies. Styling and model change, of course, are of minor importance; objectives are rational design for repair and function, with maximum power and traction per unit weight. Design enters into coupling, with control, to plows and attachments. Speed and power increases allow an increase in the number of simultaneous operations like plowing, fertilizing, and seeding. These advances decrease the number of passes that must be made over a field in production of a crop.

Modern dairying provides examples of sophisticated mechanization. In dairying, the necessity was to replace hand milking with machines

that were not irritating to the cow. As these were developed—about three-fourths of a million are now in use—better methods for feeding and housing the cows were realized. In short, the dairy became a milk-production plant importing treated feed to supply the output of milk through the cow as a converter. The barn is designed to deliver the feed from storage to the stall, and to remove wastes while maintaining sanitation in milking. Innovation in design is continuous. The milk stool and open bucket have disappeared in one generation. It is sometimes jokingly said that the only remaining objective is to breed an animal that need not be milked on Saturday and Sunday.

Processing and chemical industries related to agriculture are adjuncts to the farm. The fertilizer industry is one of the oldest and largest of these. In 1964, farmers in the United States spent about \$1.7 billion for fertilizer and lime—an increase of about 50 percent in seven years. The fertilizer industry is closely interwoven not only with farming, but also with mining, natural-gas production, and transportation. It long was based on use of superphosphate, made by adding sulfuric acid from Texas sulfur to Florida phosphate rock. The superphosphate was mixed with mined potassium chloride from Europe and nitrogen compounds from South America. The industry was revolutionized between 1930 and 1950 by development of synthetic ammonia processes to supply nitrogen compounds, and by the discovery of abundant sources of potash in the United States and Canada. The nitrogen processes rested on the discoveries of Fritz Haber in Germany, in 1913, for catalytic combination of nitrogen from the air with hydrogen, which currently is obtained from natural gas.

The farmer, as a consumer of the fertilizer, has limitations on material. These center on cost and ease of handling. Liquid ammonia as a source of nitrogen, for instance, is now added directly to soil rather than after combination in salts like ammonium nitrate, which increases costs. Potassium sulfate is produced for use on special crops such as tobacco, for which the cheaper chloride is objectionable because it changes smoking quality. Plant food content of fertilizers increases each year to reduce transportation cost and bulk of material to be handled on the farm. Organic nitrogenous materials are fashioned to permit slow liberation of water-soluble nitrogen compounds as is desirable for grasses. While these are not economic for the farm, they are achieving wide acceptance in the market for lawn use where cost is not carefully assessed. Development from inception to use has generally taken about ten years.

Manufacturers and the U.S. Government both do research and development work on fertilizers. The Tennessee Valley Authority is the governmental agency involved. In fertilizer production, it has been concerned chiefly with new products of phosphorus and nitrogen and with process modification. The Tennessee Valley Authority seeks to advance the

social well-being of a formerly depressed region by increasing farm productivity through use of fertilizer and lime, as well as by soil-conservation measures. Manufacturers are also much concerned with process modification. The two, which were at one time in open conflict, keep a wary eye on each other, but friction has diminished over the years. The fertilizer industry has come fully to realize that it, too, serves the public interest and that there are social ends in fertilizer use that fall best into public categories. It is a high-volume industry with low margins of profits. The many small companies are now being integrated into larger petroleum companies. This move enhances research development by concentrating research effort. It strengthens the industry while reducing costs to farmers.

Public research on fertilizers is devoted chiefly to methods of use. It is largely done by State Agricultural Experiment Stations, including the national Cooperative State Research Service (CSRS). The research involves the types and amounts of fertilizers to use at various times in rotation of crops in particular fields. The present trend is to increase amounts as greatly as the growth of the plants will permit, but each case has to be carefully examined. Possibly the greatest increases in yields per acre are now achieved through increased use of fertilizer. The yield per acre of cotton, as an example, has about doubled in the last 15 years. In the case of corn also, much previously unfertilized land in the midwestern corn belt is now fertilized at increasingly heavy rates. The commercial fertilizer consumption in Illinois increased tenfold in the last 25 years, to about 2 million tons, and in Nebraska the increase was about 270-fold, from 2,100 to 458,000 tons.

Research on plant nutrition, which underlies fertilizer usage, is also done chiefly by public agencies. Striking returns have been obtained from the research in some nations. An example is Australia, where lack of molybdenum, which can be readily supplied, limited clover growth. Recognition of deficiencies in plants arising from low levels of elements required only in small amounts has also led to use of the elements in question in many areas of the United States. Thus, iron is supplied to citrus in California and to pineapples in Hawaii. Copper salts are needed on peat lands in Michigan. Many beet- and celery-growing areas need boron to prevent poor plant development. The feeding of plants, while not as demanding of attention as the feeding of babies or chickens, needs to be done with care.

Special nutritional needs may exist in some regions. These can be delineated only by surveys. The surveys often deal with minor nutrient elements required by animals and men rather than plants. Selenium, which is involved in muscle development, is a case in point. It is required in small amounts, but is toxic to animals when present in large amounts.

The areas in which it is below required levels are partially known, but a complete survey is still needed. Areas in which it is in excess also are not fully defined. Appreciation of chromium and several other elements is just now developing.

Insecticide, fungicide, and herbicide development is another large area of industrial and public effort. The division of work is somewhat the same as with fertilizers. Industry largely develops the particular compound and works out production as well as formulation for use. The public agencies work out the methods of use. They also assess the hazards involved to the user, to the plant or animal being treated, and in polluting the surroundings. The public agencies are responsible for hazards to the consumer and must evaluate the extent to which these are real. There is great ferment in these areas.

Food production depends heavily on pest control, and pesticides are an effective means of achieving this control. In underdeveloped countries, which in general are tropical, pest control and fertilizer supply are prime needs. They underlie successful production, but obviously must involve a system of supply and distribution, whether by importation or national manufacture.

The time from recognition of possible action to wide use of an effective agricultural chemical is the order of eight years in the United States, as based on examples of DDT, aldrin, and 2, 4-D (a herbicide). Steps in the development process are: (1) discovery of a compound, which might involve a reasonable amount of chance, (2) assay for activity, (3) modification of structure to enhance activity or reduce toxicity to the animal or crop involved, (4) formulation for application, (5) testing on a small scale, (6) toxicity tests on residual materials, (7) manufacture on a pilot-plant scale, (8) test on a large scale, and (9) design of plants for production, provided the entire development is economic.

Research by the public bodies also is most effective over the initial eight-year period. It begins when the compound in question is available in small amounts. Thus, work on DDT in the United States for control of the louse on humans started shortly after a small amount was found in packets supplied German troops in North Africa. First steps are measures of effectiveness against particular species, that is, on specificity. This is followed by assessment of dosage rates and periods over which a given dosage is effective. Tendencies of the pest to adapt to the toxicant are measured, as are possible injury to the plant or animal being treated. The actual way in which the compound can be used is next worked out, often in cooperation with the manufacturer.

As usage increases, questions of residues and pollution, or of increasing tolerance, arise. These have often been faced only as they become apparent. Because of the seriousness of the questions raised and of possible

effects, this procedure is being greatly modified. Now the hazards must be more thoroughly evaluated before introduction for general use. Legislation is not yet complete in this regard, but each question raised advances the progress toward complete coverage. An estimate of the additional time to develop use because of regulation could be the order of five years.

Innovation in the pesticide field has been rapid since the discovery of the first striking cases. Regulation, viewed as a cause of slower development, comes at a stage of progress in the introduction of such materials when it will not be so seriously felt. It will probably restrict usage of compounds that are near the economic limits and consequently cannot bear the higher development costs.

Pest control other than by use of pesticides is done almost entirely by public agencies. It is a whole complex of biological work involving identification of the pest (taxonomy), its ecology, epidemiology or conditions for rapid increase, parasitology, nature of invasion of the host, effect of pest on the host, and genetics of the relationship of the host and the pest. Much of the work centers on the nature of the host and the pest, and becomes mission-oriented for control only at a late stage.

Control of the screw-worm fly is an example of the biological approach. This fly lays its eggs in open wounds of animals and the maggots that appear eat the surrounding flesh. The flies overwinter in warm regions and spread outward in spring and summer. Populations are generally low, but are successful in finding a host. The idea for control, which originated in the 1930's, was based on the fact that each fly breeds only once. The idea was to produce sterile males and release them in adequate quantities to swamp the normal male population. Several such releases, if successful, could quickly reduce the number of fertile matings and the procedure could be pressed to the vanishing point for the fly.

Males can be sterilized by X-rays. Many millions are required for release over tens of thousands of square miles, if control is to be effective. A plant was built for maggot production and sterilization. The treated flies were then released from planes in the desired pattern. Completeness of control was measured by trapping the flies on baits to which attractants were added. Complete control was attained in Florida, the fly level being reduced to zero. Similar measures are in use in Texas, the other seriously infested area. However, Texas is subject to reinfestation from uncontrolled areas in Mexico. Mexico, by agreement, shares in the program, but its infested area is very large. Control, rather than eradication, is the hope.

Study of the screw-worm fly covered a period of about 20 years before use of the sterile control was attempted. The actual control program became effective over a period of about five years. It was beneficial in that the private saving from eradication greatly exceeded the public cost.

Other biological approaches to insect control are by parasitism with other insects and by spread of insect disease. An example is control of red-scale insects on citrus by parasitism with a wasp that lays its eggs in the scale insect. Another is the use of bacteria to produce the milky disease in Japanese beetles. If the controlling agent persists in the environment, a balanced stage is reached with the pest. While the pest is still present, it is kept below epidemic levels.

An innovation in insect control is use of pheromones or natural attractants and repellents. The insect recognizes its mate and its host by some property, often a volatile compound. The recognition mechanism is amazingly sensitive, possibly going as low as a few molecules in a cubic millimeter of air, or, in terms of distance, hundreds of meters under favorable conditions. An approach to control is to isolate and identify these pheromones and then to work out a synthesis of the same or of modified compounds that are still effective. This has been done for a sex attractant of the gypsy moth, a serious pest of forests. The next step is to combine the attractant in some type of control system. A possible way is to saturate a region with the attractant to confuse the male in finding the female. Another is to use such an attractant for a trap containing a poisoned bait or a method of destruction. This last method has been widely used with moderate success, but it is tedious in requiring attention to the traps.

Biological methods of controls are also based on principal attention to the host rather than the pest. An example is control of bean root rot on 600,000 acres in Michigan, the principal bean-producing region. The rot is caused by a soilborne fungus that attacks not only bean roots, but also roots of other plants as well. To treat all the soil in question with a controlling chemical would be uneconomical, even if an effective chemical were known. Plants show little resistance to the fungus in question, so breeding does not hold much hope. Instead, a study was made of the conditions under which the rot became serious—an ecological and population question. This was first restricted to test plots of the Michigan State Experimental Farm at East Lansing. Crop rotations are used on these plots and in the fields. A correlation was established between the sequence in the rotation and the seriousness of attack. Attack was the least serious if beans were planted following corn. The next step was to question the farmers. This established the same correlation. So the practice was recommended to plant beans only following corn. This has resulted in reducing the losses to a reasonable point. These studies extended over a period of 20 years. Their success depended on close contact of the Experiment Station research staff with actual production on individual farms through the farm agents. The method is an approach to population dynamics without resolving the problem into its parts, but rather proceeding by changing major variables.

Control of plant disease is a major struggle in agriculture. Losses are seldom below 10 percent of production and they have been and still can be catastrophic. Stem rust of wheat is one of the ever-threatening diseases. It develops as pustules that break through the leaves and stems of infected plants. Myriads of spores are released and are carried by the wind to other fields. Some of the spores are blown southward from the Northern Plains. They overwinter on wild grasses and, when the weather is favorable again, multiply and are dispersed northward where they reproduce to epidemic levels.

The rusts have complex life histories. Wheat-stem rust first produces red spores, which are followed by black ones as the plant matures. The black spores infest grasses on which they produce small colorless spores, which can infect some species of barberry plants. They reproduce sexually in the barberry plants and thus produce new genetic types.

Control is achieved by breeding for resistance. Wheat varieties from various parts of the earth have a varied pattern of resistance. These are collected and maintained in a region far removed from infection. There are many hundreds of known rust strains and sexual combination can produce new ones. In a particular area, however, only a few strains are dominant. Breeding is essentially seeking or producing the plants resistant to the dominant rust strains. When resistant plants are found, they are bred to incorporate other qualities, and finally, seed are increased for general use. After a time in use, either different rust strains invade the area or new strains arise. Because of the latter, a concerted effort is made to eradicate barberry. Eventually, though a new wheat variety is needed. The time for the whole process from initial breeding to final decline is on the order of 15 years. A potential backlog of varieties showing varied patterns of resistance must be maintained at all times.

The pattern of research used with wheat-stem rust is also followed with the seven other dominant rust diseases of wheat and other cereals. It is also followed for fungal diseases such as smuts of cereals, rusts of beans, cotton wilt, yellows of cabbage, and the late blight of potatoes (the cause of the potato famine of 1846-1848 in Ireland).

The struggle against fungi, which infect plants and animals, as well as bacteria and viruses, deeply involves the environment. For this reason and because of the hazards of frosts, hail, drought, and rain, agriculture was early concerned with climate and weather. To meet the need, a weather bureau was established in the Department of Agriculture, after initiation in the Smithsonian Institution. In time, it became a full, independent, and most essential public activity. Weather is a crucial factor in spread of fungi that develop rapidly under warm, wet conditions. An understanding and measurement of the factors involved, coupled with a warning service, allows farmers to take protective measures for preventing epidemics. Applications of this type are often realized over a course of a

decade after a reservoir of information is available. They require a survey and reporting system as well as a warning network, which is now largely based on radio.

The farmer is not hopelessly at the mercy of weather. He can mount a defense against some frosts, as is done by heating groves in citrus-growing regions. Cranberry bogs can be protected by flooding upon warning.

The farmers can even mitigate the effects of random droughts, provided sufficient knowledge is available about the statistics of occurrence. Irregular irrigation by overhead sprinklers can be profitable if the value of the crop per unit area is moderate or high. In irrigated regions and on land where water supply for a season can be marginal, as in much of the wheat-growing area, knowledge of water in the soil or on the collecting area is valuable. Water can be preserved by fallow and captured by terraces.

Agriculture has a primary concern with water, both as necessary for crop growth and as a destructive agent in erosion and floods. Engineering knowledge is required for the most effective practices. Erosion depends on the type of soil, the slope, the amount of water flowing, and the type of cover, such as grass or row crops. Selection of management practices for a given type of farming are based on the possibility of erosion. There are always compromises for protection against common maxima of runoff, rather than extremes, balanced against costs. The research takes the necessary compromises into account and includes the interest of the public in prevention of floods in distant streams and silting of stream beds. It assesses long-time as well as immediate needs. Correct control measures are often possible only when subsidized.

Salt accumulation is a major problem in many irrigated areas of the United States and is catastrophic in some nations (West Pakistan and Iran, for example). Waters contain salts that are concentrated on the land by evaporation accompanying plant growth. Excess water and adequate drainage must be provided if the salts are to be prevented from accumulating. Cultural practices can contribute to the solution of the difficulty. Also, initial attention can be turned to use of only suitable land for development. Segments of public interest are often opposed in the use of water, as in our southwestern states, and these uses require continuous examination based on new knowledge and developing circumstances.

Reasonable solutions can be reached for water requirements of agriculture in each region of the United States. These might require billions of dollars for construction of storage and conveyance structures. They might require revisions of laws related to water use and to segmentation of authority to small districts. In some arid regions, it might be best to give up agriculture in favor of alternative water uses. In any case, the facts should be established and ways developed to meet the needs.



Perhaps enough has been said to afford some insight into the breadth of agricultural research, its industrial ramifications, and its concern for the individual farmer as well as the national interest. The public research is both concentrated and dispersed—concentrated where intense developments of knowledge in particular areas are involved, such as genetics, parasitology, and hydraulics; dispersed where the soils of a region require examination, or where an epidemic in plants or animals is involved.

State and National Government functions are usually distinct and clear-cut, although these functions may overlap in part because of excessive ambition of research or development groups. In general, action groups for correcting or improving local conditions are state-operated save when these approach disaster proportions, as in major floods or epidemics. Research that rests on accumulation of great quantities of background material, and information that serves many regions is a function of a central organization. Commercial or industrial research functions come when the product of the farm or the material used by the farmer move in commercial channels.

Agricultural research developed early in the United States, much of its first return was from use of very elementary, but little understood, principles, when investment in all research was small. Sophistication has increased over the years. Funds for public agricultural research have increased moderately in terms of constant-valued money, but they have not burgeoned as have others in the last 15 years. The increases are often in support of limited subjects where the economy of a region is involved as in cotton or tobacco production, or where public concern is intense, as with pesticide contamination. It is difficult in the public sector to establish true priorities divorced from pressures. Thus, means of pest control possibly merit greater public support than does work on pesticide contamination, which is an adjunct to it.

In environmental types of research, where knowledge is required of soil, water, crops, pests, and climates of particular regions, it is difficult to establish and maintain adequate research teams. A laboratory established in some locality might in time better be shifted elsewhere or its facilities used for an entirely different problem requiring different personnel. This is a management question that could be handled under adequate authority, but which tends to be identified with congressional districts. While economic controls become more centralized with time, research in agriculture tends to be decentralized. This decentralization can delay acquisition of some of the more essential knowledge, as on the nature of viruses and the principles of regulation of growth. Obsolescence of research is the greatest danger.

Redirection of research groups is difficult in both the public and industrial sectors. This is true because, in the end, a group must redirect

itself, for it knows best what its concerns and capabilities are. Unless re-direction is achieved, a mission's public objective may be lost or a company may decline.

Attention of the public was recently focused on overproduction of some crops and their subsidized support. This situation was used as an argument against continued research and development, which was correctly identified as a cause. In agriculture though, as in industry, research success is the best justification for research support. The production capacity and the research know-how in supplying wheat was an asset when the need suddenly arose for relief of potential famine in India. The United States and all other nations are too close to inadequate supplies of food to risk containment of economic and technologic growth.

Those concerned with bringing new or improved technologies into effective use by any group can profit by study of the successful pattern followed in agriculture. This pattern, in résumé, affords the opportunity for intensive effort on technology while ensuring use by individual producers. The unorganized producer has a readily accessible organization to which he can turn for necessary advice. The technologist has an effective organization for putting his findings to work. No section of the United States, however small, goes unconsidered. A reasonable channel is afforded for bringing technologies into use or for capitalizing on technological transfer.

## ECOLOGICAL BIOLOGY IN RELATION TO THE MAINTENANCE AND IMPROVEMENT OF THE HUMAN ENVIRONMENT

by G. EVELYN HUTCHINSON

The present contribution will be devoted to an exploration of the difficulties encountered in trying to apply modern knowledge of ecological and evolutionary biology to promote human welfare.

The concept of human welfare will initially be taken as undefined, in the sense that everybody thinks he or she knows what it means. Later some more explicit ideas of what welfare might mean will be developed.

### Nature of Ecology

It is first necessary to say something about the kinds of science the application of which is the subject of the essay. Ecology is defined as the biological science that considers the environmental relations of organisms. The word is, in fact, often bandied about by people who have discovered that they do not inhabit vacua and are euphoric about their discovery. More seriously the word covers two more or less separate disciplines. One, often called *autecology*, is concerned with the relations of individual organisms to the various factors into which their environment can be dissected—its temperature, pressure, chemical composition, physical dynamics, and capacity to produce suitable food. Most of this subject is an extension of physiology. The autecology of man is obviously of the greatest importance in public health and medicine. It is now undergoing a remarkable development in the study of man in space. For the most part, however, it remains a kind of physiology. The other division of ecology, which has traditionally been called *synecology* (*biocoenology* or now *systems ecology*) deals with communities or parts of communities. Its essential feature is that within its view are many organisms interacting, with each other and with the environment. Much of the interaction involves autecology, but there are usually ways of studying synecology at a higher level than the single organism.

We can in such studies consider the entire earth with all its organisms together as a unit. This may sound over-ambitious. For certain purposes, however, it is very important to make, for instance, an estimate of how much sunlight falling on the earth is captured by green plants, how much oxygen and how much organic matter as a whole they produce irrespec-

tive of the kinds of plants involved. The significance of this kind of question appears in the first example, given below.

We can on a more limited scale study a community, a forest, a desert, or a lake, either arbitrarily or naturally delimited, finding out how much energy goes in and what happens to it. This can be done by rather broad categories, all green plants considered together, all plant-eating animals, all carnivores, all decomposing bacteria or fungi. Alternatively we can look at a group of species, trying to keep track of all individuals over a limited area, or even of one species in a single population, either in nature or in the laboratory. These methods of approach usually involve various kinds of short-cut methods that are useful if only because we usually can not hope to keep track even of all the plants and animals visible to the naked eye, let alone the microscopic forms. In all such work, which has been pursued with great success as a branch of unapplied biology, it has often proved useful to make mathematical models of the systems under consideration. This has led community ecology or synecology to develop a considerable abstract theory, in part equivalent to that developed in the closely allied field of population genetics. These areas, in fact, were the first and still are perhaps the only areas of biology to develop an autonomous kind of theory that is not just low-level theoretical physics, engineering, or physical chemistry imported to do a job on biological material. The importance of this theoretical structure may seem a little negative. Experimental confirmation in the laboratory is usually possible after a fashion; this means merely that we can use reproducing organisms as the moving parts of extremely bad analogue computers by which the properties of differential equations can be investigated in a very inefficient way. Often in nature the predictions of theory are not confirmed. This tells us that at least one of the simple assumptions underlying the theory is incorrect; this may be an extremely important discovery. On the whole this mathematization of community ecology has been abundantly justified, but its limits must be clearly understood.

We have, then, in synecology a subject of great scope, considerable sophistication, and at the present moment little apparent practical utility. Before considering the details of this apparent lack of utility it is worthwhile looking at another allied, in fact interdigitated, branch of biology, namely, the study of evolution. This study, which in a serious scientific sense is now 108 years old, has probably had more effect on the outlook of mankind than any other branch of science. In so far as evolutionary biology has led mankind to a more truthful understanding of his condition and problems, its impact must have been beneficial. It is for instance, becoming clear that Freud's initial interest in Darwin laid a foundation for the development of psychoanalysis, and it is not impossible that part of the unsatisfactory nature of that field today lies in its dissociation from a biological point of view. Admittedly the impact

of evolutionary thought, though immense, may still be inadequate; it is quite hard to find in the writings of most philosophers or humanists any real understanding of man's relationship to the rest of the living world. Yet it will probably be apparent to any intelligent observer that no single change in outlook has been as great in the past millennium as that which was started by Darwin and Wallace in 1858. Curiously enough, up to the present the theory of evolution, as distinct from genetics as a tool in creating new races of domestic plants and animals, has been essentially unapplied. It has been misunderstood, largely willfully, and been made an excuse for appalling racist crimes, but the beneficial application remains for the future. This is partly because, *in detail*, evolutionary theory has as yet virtually no predictive power. We can say that, given a certain sort of natural selection, a particular variation will become fixed or continue to persist in the population, but there is no way yet, from the study of the properties of the variation, of predicting the nature of the selection in a given population or community. If this is ever achieved, it will be through a very refined type of ecological study.

We may perhaps suspect that part of the practical utility of both ecology and evolution consists in providing ways of looking at large problems, and in providing fruitful analogies to other sorts of human affairs. This is indeed partly true, but it would be very odd if detailed knowledge of the workings of the systems in which we live, move, and have our being, were not of considerable direct use to us. We may begin by looking at an example.

### Some Aspects of the Ecology of the Atmosphere

The atmosphere of the earth consists, at low levels, of a mixture of gases of which nitrogen (78 percent by volume) and oxygen (21 percent) are the most abundant. Among the minor constituents are argon and other inert gases (about 1 percent), a small amount of carbon dioxide (about 0.03 percent) and a great variety of substances in very small amounts, though their effects are far from negligible (1).

The atmosphere, over water, is not in thermodynamic equilibrium; that is to say that it could change spontaneously in a specific direction, though very slowly in the absence of appropriate catalysts or activators. Since, if this happened irreversibly, our rivers and lakes would become dilute nitric acid, which would be most inconvenient, Lewis and Randall, in their famous textbook, *Thermodynamics*, expressed a hope that nature would not discover the activators. Actually nature discovered them long ago; small amounts of nitrogen are continually being fixed photochemically, electrically, or biochemically, and much is later oxidized to nitric acid, which forms nitrates. Our whole life, in fact, depends on the

process, which of course we have learned to accelerate. Only in very dry areas do salts of nitric acid accumulate, for the whole process is cyclical and very complex. Among other compounds, small amounts of nitrous oxide ("laughing gas") are formed, a fact that was discovered independently by an agricultural chemist looking at soil bacteria and an astronomer looking at the spectra of sunlight inevitably filtered by the air, but it took some years before anyone put the two contributions together, which shows how slowly the subject can advance in the face of intellectual isolation.

Of the less abundant gases, none is more important than carbon dioxide, for it is the splitting of this gas in sunlight by green plants that gives us the carbon compounds that we eat and the oxygen that we breathe. Moreover, in the past all the fossil fuels that we burn were produced by this process, and in burning they remove some oxygen and restore some carbon dioxide to the atmosphere.

About 20 years ago, evidence began to accumulate that the small amount of  $\text{CO}_2$  in the atmosphere has increased somewhat since 1900, now perhaps by 10 percent, from, say, 0.029 percent to 0.032 percent by volume. This is not the sort of change that would appeal dramatically to the public who breathe the air, but it was large enough to interest a few geochemists. The observations were mainly European, but if they were representative of the whole easily mixed lower atmosphere (troposphere), the amount of additional  $\text{CO}_2$  could be calculated, and it turned out to be about the amount produced to date in this century by the industrial burning of coal and oil. Shortly after the second World War, W. F. Libby made his discovery of  $\text{C}^{14}$  dating. It then seemed interesting to see, from a study of wood of known ages, whether the burning of coal and oil, which contain no  $\text{C}^{14}$ , had diluted the atmospheric natural  $\text{C}^{14}$ , which is formed continually by cosmic rays, only to decay slowly by radioactive disintegration. It turned out that a small dilution of the order of 3 percent had occurred, but that this is significantly less than would be expected (10 percent) from the amount of industrial combustion (Suess effect). Evidently the greater part (about 60–70 percent) of the carbon dioxide produced by industry leaves the air and goes somewhere. If it went into plants it would dilute the  $\text{C}^{14}$  of modern wood; this does not happen, so we can only suppose it goes into the sea. If it goes into the sea rather than into the atmosphere, what has caused most of the 10 percent rise in  $\text{CO}_2$  in the air? The whole matter is still under discussion. It is very likely that the relations with the sea involve a fairly rapid exchange, industrial  $\text{CO}_2$  ("dead," i.e., without  $\text{C}^{14}$ ) going into the ocean as  $\text{CO}_2$  containing  $\text{C}^{14}$  diffuses out. It has been suggested as an alternative that most of the rise comes from soils that once were forested and now are turned by the plough. All these things are going on around us, but we still do not have complete information as to how important any given

process may be. We are certainly not in a position to forecast what may happen to the  $\text{CO}_2$  concentration of the atmosphere in the immediate or remote future. The problem may appear to be an academic exercise of no great human significance, but actually it might come to be of considerable importance. An increase in carbon dioxide at present may be producing small increases in photosynthetic rate and so in agricultural productivity. Many investigators in the past have felt that an increase in the concentration of the gas could cause climatic changes, as  $\text{CO}_2$  absorbs infrared radiation strongly and acts to some extent as a greenhouse enclosing the earth. The most modern studies suggest that the important results of an augmentation would be a slight increase in cloudiness, and so an increase of back reflection of sunlight into space, offsetting any increase in temperature in the "greenhouse" (2). It is evident, however, that we do not really have enough information to predict what would happen, though since we live and breathe the atmosphere, even its slight alteration might have significant practical consequences. The barrier to application of knowledge in this case is simply that we do not have enough knowledge to apply.

### Responsibility, Understanding, and Economics

There are certain general features of the foregoing example that are of particular interest in the present context. It is to be noticed that the subject has appeared relatively unimportant because the measurable changes so far are very small and entirely harmless. Moreover, we are imbedded right inside the system, which is very much bigger than we are; to use old-fashioned engineering parlance, the intensity factor is very small, the capacity factor enormous. We do not have really satisfactory figures on the over-all rate of photosynthetic carbon dioxide fixation and oxygen production for the earth as a whole, nor for the movement of carbon dioxide from the air into the bicarbonate of rivers eroding rocks and so into the sea, nor for the production of the gas by volcanoes. All these figures may enter into the over-all balance, but they are not now well-enough known for the finer detail to have any significance. The same sorts of difficulty would characterize any applied ecology of this sort.

Moreover, in any particular problem, the actual practical solution will generally consist in finding key processes that can be modified. The method of modification usually involves techniques entirely unfamiliar to the people who understand the large-scale processes that are being altered. Roughly, the intensity factors belong to one discipline, the capacity factors to another.

We see another example of this last difficulty in the problem of insecticides. These are mainly developed by organic chemists working in con-

junction with insect physiologists. Until the public became worried, the effect of wholesale application were disregarded because the people most involved had little experience in thinking in terms of ecosystems (3).

Problems of this kind involving a large-scale effect of small initial intensity, and with lack of sympathy between specialists, may lead to situations of considerable economic difficulty. The widespread diffusion of some particular substance or process, transgressing the areas of private enterprise, local government, or even national interest (there is evidence of lead accumulation from automobile exhausts in the surface phytoplankton, on which fish ultimately depend for food, far out to sea in the Pacific, and insecticides have somehow spread to the livers of Antarctic Penguins), will ordinarily force control measures to be taken by Government agencies or international agreement. Although the ultimate gain in such measures may be very great and universal, no one is willing of himself to take them, and some individual short-term interests are often damaged by control measures. One of the many problems of water pollution may be used as an example.

Among the numerous things that should be done to clean up our inland waters is a systematic attempt to prevent high concentrations (*high* here means over one part of phosphorus in ten million of water) of phosphate or other assimilable phosphorus from entering our lakes and rivers, where it produces excessive algal growth and consequent unpleasant and unhealthy conditions. Ideally, trapping this phosphorus by at least one of two apparently feasible methods represents also the conservation of an essential resource (4). Unfortunately, with a considerable reserve of easily available phosphate in the United States, these recovery processes are not economically self-supporting, though highly desirable. They have to be subsidized if their ultimate benefits are to be enjoyed.

The same sorts of difficulty will crop up whenever applied ecology is being directed to preserve for our use any large segment of our essentially continuous environment, be it air, inland waters, the ocean, or the mantle of living beings.

### Psychological Difficulties: Fluoride and Politics

In a few, but often very important, cases, not only are public apathy and economic interest involved, but also positive psychological difficulties have to be overcome. The whole question of the right to control the over-all environment has been raised in the case of fluoride addition to drinking water to prevent dental caries. This is presumably a question solely of the propriety of governmental (local or state) action, because the same groups do not conduct campaigns against the private introduction of lead, carbon monoxide, sulfur dioxide, and other potentially harmful materials into the atmosphere, or equally poisonous materials into the hy-



drosphere, on a large scale. This case is an instructive one because the biological and geochemical aspects of it were unusually simple and the technology presented no insuperable difficulties to waterworks engineers. Although aspects of the mineralogy of teeth and bone still require research, practically all the opposition to fluorination seems to have been based on specious arguments that could have been applied to prevent, for instance, the Quartermaster General allowing Army cooks to use salt. In this case, public policy encouragingly shows that the electorate was not over-impressed by such arguments, even though they may not have realized in detail quite how silly and dishonest some of them were (5).

### Intellectual Difficulties: Understanding the Population Explosion

We might also consider the vastly more important problem of population growth which is at the bottom of most of the other problems of applied ecology. This case is instructive because the difficulty seems to lie not entirely with the reluctance of the general public to regard itself as members of a biological species, but partly with lack of intellectual breadth on the part of the specialists involved.

It has been suspected since the time of Malthus that all human populations tend to increase, and it has been hoped since the work of Verhulst, first published in 1838, that the *rate of increase* may tend, after a time, to decrease, so that the population has an upper limit, approached by an "S-shaped" curve. Since the 1920's, it has been shown that very roughly most human populations have grown in this way, though changes may occur restarting the process toward a new upper limit. Concomitantly a vast amount of information has been accumulated showing just this kind of population growth in bacteria, protozoa, insects of many kinds, as well as various vertebrates. Clearly the growth of *any* population can, at a first approximation, be represented by a model in which the increase is proportional, on the one hand, to *the number of individuals that can engage in reproduction* and, on the other hand, to the number of "*vacant spaces*" available. The "number of vacant spaces" may vary of course, with the conditions; man has in the past had no difficulty in making more, but it is also quite likely that a saturated population is an over-optimal population. These qualifications, however, do not remove the basic proposition that in the model the "vacant-space" term exists; moreover, in the growth of the U.S. population, the upper limit with no vacant spaces certainly does not correspond to "standing room only." The question that is usually evaded: How does the rather abstract *vacant-space* term work? The answer at the moment is that in general for man we don't know much. There is some quite recent evidence that urban crowding can reduce fertility. Yet it would seem obvious that such a vacant-space term is likely to be of consid-

erable practical importance. This failure to know is probably largely due to a failure to transfer concepts from one branch of science (comparative animal ecology) to another (human demography). It is essential to bear in mind simultaneously both the resemblances and the differences of the situation studied by both sciences, in other words, the resemblances and the differences between man and other animals.

More recently, ecologists have given a great deal of attention to the other term in the model, the proportionality between rate of population increase and number of reproducing individuals. In its simplest term this depends on a proportionality coefficient, given by the birth rate minus the death rate. The "population explosion" is fundamentally due to the fact the death rates have recently, due to medical advances, fallen below birth rates. The question of what sets the birth rate, however, is less obvious. It is clearly always partly determined socially, for no community ever achieves its maximal biological reproductive potential. It can be reduced obviously by contraceptive practices, but it is a curious fact that countries exist in which the birth rate has increased since such practices became widely available, and that in the United States, in some states at least, a negative correlation of family size with income has been replaced by a positive correlation indicating that a certain-size family has become a social value. *No amount of dissemination of information on birth-control practices is going to produce a stable population if the members of that population insist on a birth rate that is inconsistent with stability.* Yet such information as is available suggests that that is exactly what they are trying to do in this country at the present time (6).

The comparable problem of setting optimal birth rates by evolutionary processes has been of great interest to animal ecologists. An ecological look at the practical problems of human population would undoubtedly have led to greater attention being paid to the matter much earlier. It is curious, however, that in man the intellectual solution of the problem and *the application of the solution* would probably be in purely sociological terms, though the *identification of the problem*, which comes so easily to animal ecologists, has until recently largely escaped sociological demographers.

### Science, Government, and Education

The examples given here illustrate several conclusions that could be drawn from any broad survey of the field.

1. There are a number of areas, relating to such things as changes in the composition of the atmosphere, or to mention another example, ongoing human evolution, in which we simply do not have enough knowledge to apply, however important the applications may be. The

only cure for this situation is more basic research; fortunately the kind of problem involved is generally felt to be extremely interesting and, provided no significant reduction in support occurs, the applicable knowledge should develop in the foreseeable future. This is the kind of work best left largely to universities.

2. The initial low intensity of important ecological warnings, the fact that we are completely embedded in the ecosystem, which knows neither state nor national boundaries, and the disinclination of many private operators to cope with the extensive efforts of their operations, necessitates a considerable degree of Federal control, supplemented by various international activities. This is widely recognized in agriculture, fisheries and meteorology; it is becoming acceptable in the case of air and water pollution. In a democracy such acceptability depends on a broad elementary understanding of the principles of environmental interaction. *Application of what we know depends therefore on an adequate amount of education in ecological thinking.* Experience seems to show that where the empirical connection between some activity such as fluorination and the resulting benefit (in this case, prevention of dental caries) is sufficiently well established and the value of the benefit sufficiently obvious, no great obstacles to acceptability exist, even if a minority opposes the action on unscientific and in part quite perverse grounds. A greater difficulty may have to be faced in the case of population control, where most people of reproductive age in the United States appear to accept contemporary contraceptive techniques, but do not accept the need to limit the average family to an equilibrium size just permitting replacement.

3. In a number of specific fields, notably in water pollution where very extensive economic interests may let desirable changes go by default or even generate strong counter pressures, development of existing research and the search for better, more economically feasible techniques certainly justify very intense Federal activity. This should include studies of tax incentives and other socioeconomic methods of persuasion as well as purely technological study. It cannot be too strongly stressed that applied ecology, being fundamentally everybody's business, is far more likely to end up by being nobody's business than is any other kind of applied science. In this area, Government research institutes, in close touch with practical needs on the one hand and with legislative and executive agencies on the other, probably will play an important role.

4. There can be very little doubt that a major factor in the unsatisfactory nature of much applied ecology, results from a failure of communication between workers in different but contiguous fields. In both the insecticide problem and the problem of population control this, as has been briefly indicated already, has certainly delayed progress. The fault here is primarily with the individual workers and their training. There has been too much emphasis on the intensive compartmentalized point of

view and not enough on an extensive global vision. A greater number of very well-educated applied ecologists of marked ability would help here. This need can be satisfied only by an emphasis on the enormous contribution to human welfare that such people could make. The kind of attitude that seems needed is developed in the following final section of this essay.

### What We Should Aim for: "per ardua ad astra"

Within the whole field we may recognize a general kind of antithesis, typified by Alvin Weinberg's conception of the "technological quick fix" and the so-called "scientific backlash." The development of modern insecticides was probably an important "quick fix" procedure in increasing food supply, but the slow poisoning of the environment in the "backlash" must be taken very seriously indeed, and indeed now can be, since the initial processes bought off famine and so gave a certain amount of time. As in most human activities the process is dialectical. Parenthetically it may be remarked that the dialectic is far older than Hegel and Marx, beginning when the first youth revolted against his father and then realized in the light of his own experience that some of the things, though not all, that the old man was talking about made sense in a new context.

It is probably necessary in all such dialectical processes, involving a balancing of interests in a new synthesis, to look at the values involved. It is here that perhaps we shall find most hope for a really beneficial applied ecology. If a sufficient number of able people is to be involved in any kind of scientific application, *the involvement must be in something that really seems worthwhile*. The idea of biology we are considering may be able to lead the way to some extent in this. It is virtually never out to sell a product in a simple-minded sense. It is clearly often the guardian of the environment, it might aim systematically at the improvement of the world.

It has been said that the main difference between the great and the mediocre musical performer lies in the former having a clear and detailed understanding of exactly what he wants in a performance. The same can probably be said of many other activities. From this point of view the applied ecologist should be thought of as the designer of means to realize a desirable world, given the material and the limitations imposed by the nature of the universe. (By this I mean not human nature, which we know to be mutable, but such things as the conservation of energy, the second law of thermodynamics, and the upper limit of velocity at the velocity of light.)

It is not fashionable at the moment to engage in scientific optimism. We have been discouraged by Hitler into completing the triumph of evil by supposing that since many millions of people have been murdered in the 20th century, there is nothing left to do save to lament the degeneracy of the time. Ideas like the Great Society and the abolition of poverty are

good but very simple-minded beginnings not yet powerful enough to counter the post-Auschwitz malaise into which intellectual, particularly non-scientific intellectual, society has fallen. Fortunately there are some signs of a return to a critical and constructive optimism, but a quite cursory study of contemporary literature and art shows an emphasis on alienation, which in an extreme form is the antithesis of the thesis here being developed. We must go much further than merely proclaiming a good society in a good environment, we must show in detail how it can be achieved step by step over a long period of time, but fast enough never to let despair disrupt the building process.

The job of applied ecology, working with the applied aspects of the sciences of man, is to produce initially a practical scheme attractive enough to draw people into its realization.

At the beginning of this essay it was assumed that the welfare that applied science should produce is reasonably easily recognized by each individual making a decision in any given case, and that in general individuals will decide according to their own experience and desires. This assumption, however, is clearly inadequate. It makes no allowances for conflicts of interest or varying degrees of altruism, nor does it consider hidden unconscious desires that may warp rational judgment. In order to arrive at a more satisfactory position it has seemed best to compare two very extreme possibilities, one of which nearly everyone would consider bad, the other of which would almost universally be considered good. The first would in current phraseology be termed a *dystopia*, the second a *eutopia* (7). It is of the essence of the argument that neither is a *utopia* in the strict sense of the word, meaning nowhere. Each possibility is based on existing easily recognizable trends, and all the elements are either simple and not too extensive extrapolations of existing technology or are based on the characters and actions of existing people. All the human parts needed to make either model are known to exist, though not necessarily in the required proportions. With these conditions the only further restriction is that no recognizable logical inconsistencies can be allowed; the problems of one such inconsistency will be mentioned later.

The first, the dystopian or evil model, is familiar. Population increases at its present rate, there is much famine, cities become enormous, all amenities decline, racial strife and violence increase, finally either nuclear world war breaks out as a result of hunger or desperation, or a new epidemic arises comparable to the Black Death; or even if these alternatives are avoided, a dictatorial society of dehumanized human beings is established as the only possible way to avoid the other types of catastrophe. The possible details are all too familiar to the educated modern public and need not be examined further.

It is more interesting, though still much less fashionable, to examine the alternative, the eutopian or good model. Human population is sta-

bilized at some figure not greatly different from that at present, perhaps the stable number would ultimately be rather lower than today. It consists of very diversified groups of people, who are physically beautiful, though of various skin colors and physiognomies, loving, cooperative, and very diversely talented. It is exceedingly important to remember that some such people do exist all over the world today. The model to be designed is in fact based on what mathematicians would call a proper subset of the contemporary human population. They are all healthy, adequately fed and clothed, with opportunities for spectacular display if this is desired on special occasions. They engage in work that is interesting, always sufficiently demanding, and for those who want it, very difficult. The perennial challenge of space exploration is likely to engage a great deal of effort. There is enormous social pride in the beauty of cities and in maintaining a quite considerable area outside urban and agricultural land, as recreational areas, natural reserves and research areas. A large flora and fauna survive in such areas, and a great number of new domestic animals and plants have been evolved, partly for decorative as well as utilitarian purposes; some people might, for instance, keep highly modified birds of paradise in their back yards. The equivalent developments to suit nonvisual nonbiologists would obviously occur. Responsibility, intellectual depth, and gaiety would all be encouraged. There would be lots of mathematics, music, and dancing. Whatever contemplative or religious values may ultimately be recognized would be able to flourish. The social system would be in general informal and relaxed but with some opportunity for hierarchical ceremonial in which the important people would be comparatively unimportant in other respects. Every effort would be made to insure everyone being *primus inter quasipares*. (Note: Most members of the National Academy of Sciences could probably be beaten at tennis by their children; athletics are very convenient in handling this type of situation.) The roles of sexual and personality difference would develop naturally with great lability. Since, to preserve a stable population, child-bearing would be much rarer than today, women would have greater freedom to engage in the whole range of human activity. *It would, however, be desirable not to try to extend adult life too much*, because the value of having children about would probably seem to nearly everyone to be vastly more important than mere survival; the interdependence of long adult life and age structure in a stable population produces the above-mentioned example of a logical inconsistency in the design that must be avoided; it has considerable implications to current medical developments.

It is reasonably certain that most people would admit to finding a maximum number of values satisfied in a society of the kind just outlined. It is not unlikely that in actual practice many would be unable to adapt to it, but as it is designed to appear acceptable to people making public

statements, it probably represents a development of what we like to think are our aspirations; this is the only way in which aspirations can be brought into being and realized. It does involve quite a lot of technological advance but none seems to me logically unfeasible. It also involves an enormous addition to knowledge of how people differ, so that we can see what happens in the life histories of people who would not fit into this or any other model of a good society. In this context, it would probably be necessary to consider, among other things, the development of easily changed and possibly arbitrary customs and institutions against which young people can revolt without destroying the more fundamental values of the society. This well may be easier than would first appear. It would be essential in trying to develop the eutopian society to do everything by example, to build what we could as fast as possible so that it became something clearly desirable and desired. When that happened it would grow, probably reaching a "critical mass" when its virtues would be accepted as obvious. Actually an enormous number of activities is currently going on, activities that point in the eutopian direction though we are far from reaching the critical mass. What is needed now is primarily the recognition that it is to produce something that is universally delightful in many different ways, and not merely to abolish obvious ills, that science should be applied and new technology invented. This should be a sufficient and satisfying challenge to any one capable of meeting it.

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5. An excellent brief review of the role of fluoride in prevention of dental caries is given in West, E. S., W. R. Todd, H. S. Mason and J. T. van Bruggen. *Text Book of Biochemistry*, 4th ed. Macmillan Co., New York, 1966, pp. 1400-1403. Anti-fluorination propagandists have used as an argument the excessive toxicity of elementary fluorine (F<sub>2</sub>), which is as irrelevant to the toxicity of sodium fluoride (NaF)

actually used, as is the well known toxicity of chlorine ( $\text{Cl}_2$ ) to the culinary use of common salt ( $\text{NaCl}$ ). This argument has for instance appeared in a local Connecticut newspaper. Admittedly chronic ingestion of massive amounts of  $\text{NaF}$  may give rise to chronic intoxication, but the same is true of  $\text{NaCl}$ . The amounts of  $\text{NaF}$  used give drinking waters containing 1–2 parts per million fluoride, well within the natural range of surface waters, which is about 0.1–5.0 ppm.

6. The mean desired family size among white Americans rose irregularly from about 3.1 in 1943 to about 3.3 in 1961 (Black, Judith, *The Americanization of Catholic Reproductive Ideals. Population Studies* 20: 27–43, 1966). Admittedly with perfect contraceptive practice such figures might, if realized, be consistent with stability, for in any given case the ideal would never be exceeded but might not be attained. In practice, however, contraceptive and sterility control techniques are likely to improve *pari passu* so that unless a rather lower ideal is adopted equilibrium will remain impossible.

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## **APPLIED SCIENCE AND MEDICAL PROGRESS**

*by* MAURICE B. VISSCHER

The public at large supports scientific research in the field of medicine primarily because it hopes and expects that practical advances in improved health and longevity will result therefrom. Uneasiness exists in the minds of some segments of the public and the Congress as to whether the present methods of research management are consistent with these objectives. The most important policy question to answer is as to how the greatest over-all progress toward the achievement of practical objectives is to be made. The alternatives are not simply decisions as to how much should be spent on basic as opposed to applied or applicable research, but extend into the further question of whether and how central direction can or should be given toward specific or general practical objectives.

The better-informed segment of the public understands that useful applications of science depend upon prior discovery of basic facts upon which such applications can be made. In the medical field it is difficult to draw distinctions between basic and applied research, because in one sense all studies on components of and processes in living systems are ultimately "applicable" to medicine. There are differences in the immediacy of possible useful application to the prevention or treatment of disease, depending upon many circumstances, including especially the number of pieces of still-missing information between a scientific fact and its direct relevance to medical practice. It is a truism that in biology, including medical science, there is no "useless" information. There is only information so remote from application, because of our ignorance of other facts, that it is "useless" except as a foundation block upon which to build toward usefulness. Knowledge in medical science is frequently called "basic" if it is not usefully related to the management of some disease entity, but actually much applicable knowledge is also basic in the truer sense of the word "basic."

### **Major Problems in Health Research**

In broad categories the major medical problems (from quantitative viewpoints as to the cause of death) in the United States today are:

1. Arteriosclerosis and hypertension
2. Cancer
3. Trauma (in persons under 40 years of age)

As to morbidity and causes of disability, the major problem is probably mental health. The list of important diseases or disease categories in which there is great human interest coincides with the entire catalogue of human diseases, because, to any individual afflicted with a particular disabling or fatal disease, that disease assumes great personal importance. It also becomes of major importance to his friends and relatives.

Any arbitrary setting of priorities in the scale of urgency in the solution of practical problems in medicine is beset with grave difficulties. One might think superficially that simple statistics as to the frequency of a disabling or fatal disease entity might provide a basis for priorities in funding. But in reality certain practical problems are insoluble because of various factors. The most obvious scientific factors contributing to what one may call "practical insolubility" relate to lack of basic knowledge upon which to proceed to practical solutions. This situation exists in regard to some aspects of the three quantitatively most important disease categories already mentioned.

However, it must be pointed out that not all the problems in relation to lowering disease and death rates are necessarily scientific ones. Enough is known about bronchogenic carcinoma, for example, to allow one to predict with a high degree of probability that the prohibition of sale of cigarettes and a reduction in atmospheric pollution by carcinogens would greatly reduce the incidence of that form of cancer. There is also a statistical correlation between certain forms of cardiovascular disease and cigarette smoking, but the causal chain of events is not so clear. Additional information is required, but failure in application of existing knowledge is a major impediment today.

The leading cause of death among persons between the ages of 1 and 37 is accidents. "In 1965, 52 million accidental injuries killed 107,000, temporarily disabled over 10 million and permanently impaired 400,000 American citizens at a cost of approximately \$18 billion. This neglected epidemic of modern society is the Nation's most important environmental health problem. . . ." (1) Major improvements in this situation could be made simply by better utilization of existing knowledge. Better education in first aid, better ambulance service, better communication systems on highways, better distribution of medical personnel for management of trauma, are all feasible medical improvements in the realm of application of existing knowledge and technology. Existing engineering knowledge, if applied, would permit elimination of many hazards.

To insist that failures in application of existing knowledge are partly responsible for these huge tolls in death and disability is, of course, not to say that additional scientific study at both basic and applied levels is not called for. The above-mentioned National Research Council committees discuss several mechanisms for improvements both in accident-prevention

and for treatment of trauma. They point out that, whereas in 1963 the National Institutes of Health and other Public Health Service bureaus spent \$220 for each of the estimated 540,000 cancer patients, and \$76 for each of the estimated 1,420,000 cardiovascular patients, it spent only 50 cents for each of the 10,000,000 persons disabled by accidental injury. It suggested that the magnitude of the trauma problem warrants the creation of a National Council on Accident Prevention and the setting up of a National Institute of Trauma. The latter proposal will be considered again in this statement.

### **Is Applied Research in Medicine Being Neglected?**

In studying the question of the relative emphasis being placed upon "basic" versus "applied" and "applicable" research, a study was made of the papers presented at the 1966 Scientific Sessions of the American Heart Association. Some 600 papers were presented. Abstracts of the first 100 and the last 100, as arranged alphabetically, were read and categorized as to the three above-mentioned types of work. A paper was considered to be basic if it dealt with a fundamental problem of anatomy, physiology, biochemistry, or other field not obviously related to any particular disease entity. It was considered to be applied if it dealt in large part with observations on human subjects in relation to any disease state, including studies on methods of diagnosis, prevention, or therapy. It was classified as applicable if the focus was on the elucidation of problems definitely related to a disease entity but carried out on lower animals rather than on man.

This study showed the following distribution of papers: basic research, 15 percent; applicable research, 35 percent; applied research, 50 percent. Another evaluator might categorize work somewhat differently, but the outstanding result is that a great preponderance of work in the entire cardiovascular field is on very practical problems. It is unlikely that any other evaluator would change the proportions greatly. The situation in the other major fields in medicine has not been studied in the same way, but one's general knowledge suggests that similar proportions would be found to prevail.

### **How Have Some Major Practical Advances in Medicine Come About?**

The actual history of development of major practical achievements in medicine is instructive in pointing up the complexity of the problems. In the case of the development of the poliomyelitis vaccines, the problems have been described recently by Dr. James Shannon (2). He presented flow charts to illustrate the history of the development. He pointed out that progress toward a useful polio vaccine was completely impossible before the tissue-culture method of propagation of viruses was discovered and developed. He also pointed out that, until it was discovered that

three strains of virus (and only three) were capable of causing human polio, real success was not possible. It is apparent from his account that work along several lines essential to the practical success was proceeding concomitantly, and that in reality the production of a useful vaccine of one type was accomplished within three years, and of another more permanently effective type within five years of the time that the major basic facts had been established. Considering all factors that had to go into the development of the "applied" part, the research was accomplished very expeditiously. The virus origin of the disease was discovered in 1909. The basic research required 40 years and only three additional years were required to obtain the first useful vaccine.

Dr. Albert B. Sabin has stated the situation as follows:

The development of the oral poliovirus vaccine was preceded by many years of work which was designed to provide us with the important knowledge regarding the natural history of the viruses and the human disease. This was information that had a very important bearing on the ultimate development of the oral poliovirus vaccine that was not sought with the development of the vaccine as its prime objective. Another important development, totally unrelated to the poliomyelitis vaccine, was, of course, in the field of antibiotics which made modern tissue culture methods possible. The tissue culture methods in turn made the studies on poliovirus vaccine possible. I could mention a great many more times without which it would, of course, have been impossible to develop successful vaccination against poliomyelitis. (3)

Both the National Foundation for Infantile Paralysis and the National Institutes of Health had panels advising them in regard to funding research programs in vaccine development. Experts in virology guided the management of funding at various stages. The two agencies were not always in complete agreement as to appropriate next steps; both came out with useful results and much of the work supported by one was useful to the other. One can say with assurance that, until the end result was actually achieved, it would have been a great mistake to follow one line of approach exclusively.

Another illuminating example is the development of a vaccine for establishment of temporary or permanent immunity against rubella (German measles), which is associated with congenital defects in children born to mothers contracting the disease during certain periods of pregnancy. Dr. Maurice R. Hilleman, Director of the Virus and Cell Biology Research Program for the Merck Institute, who has made major contributions in this development, has kindly provided the accompanying flow chart.

Several points in the Hilleman outline are of interest to the present discussion. First, it may be noted that, although the association of rubella with congenital defects was established in 1941, it was not until 1949 that it was demonstrated that viruses could be successfully propagated in tissue culture. Appropriate conditions for propagation of a rubella virus that could be proven to be related to the disease were not developed until 1962. Up to this point it may be said that basic information was lacking.

Vaccine development was begun and animal tests both of live and killed vaccines were initiated immediately. Studies on man with killed vaccine began in 1964 and with attenuated live virus vaccine in 1965. Since 1962, three approaches to the production of an attenuated live virus vaccine have been followed, and evidences to date indicate that virtually no time that was not inherent in a solid scientific proof of efficacy and safety was lost in producing a practical solution as to immunization capacity.

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Dr. Maurice R. Hilleman—Rubella (German Measles) Vaccine Research

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1. Clinical Disease

Disease defined clinically in late 18th century

↓  
Role of rubella in causing congenital defect  
recognized in 1941

2. Virus etiology

Virus probably grown in monkeys . . . . . 1938-1953  
(Impractical from vaccine standpoint)

Virus reported to grow in chicken  
embryos but unconfirmed . . . . . 1942

Breakthrough

↓  
Practical cell culture technology  
Enders, Weller, Robbins . . . . . 1949

↓  
Propagation of rubella virus in cell culture  
and proof of relationship to disease .

Monkey kidney cell culture  
Parkman, Buescher, Artenstein . . . . . 1962

Human amnion cell culture  
Weller, Neva . . . . . 1962

Provides technology for growing virus, detecting  
virus, and for detecting antibody (diagnosis and  
immunity)

3. Reemphasis of importance of rubella and need for vaccine

Recognition of reservoir of rubella susceptible  
women in U.S.A. averages about 18 percent  
Sever et al. . . . . . 1964

Large-scale rubella epidemic of 1964 followed by  
great increase in abortions and defective children 1964-65

#### 4. Vaccine approach to control

Cell culture of virus permits beginning vaccine  
development 1963

Two approaches explored

(A) Live virus vaccine. Intended for children  
and nonpregnant adults.

(B) Killed virus vaccine. Intended for pregnant  
or possibly pregnant women.

##### (A) Live virus vaccine

- Objectives:
1. Induce adequate and lasting immunity
  2. Safety to recipient
  3. Noncontagious to susceptible contacts (especially pregnant women)
  4. Desirably not excreted from recipient so as to obviate possible contagion
  5. Preparation in an acceptable kind of cell for human use

##### Experimental vaccines

↓  
Tests in animals 1964  
↓  
Beginning tests in man 1965  
Continuing tests in man 1966- ?

##### Principal candidate vaccines - 1966

1. Monkey kidney attenuated and grown  
Meyer and Parkman 1966

- |   |                     |      |
|---|---------------------|------|
| 2. Duck embryo cell culture attenuated and grown  | Hilleman and Buynak | 1966 |
| 3. Diploid human cell strain attenuated and grown | Plotkin             | 1966 |

**Progress:**

1. All three vaccines induce high level rubella antibody, probably lasting.
2. All three vaccines cause no significant clinical illness in primary recipient.
3. All three vaccines are of low or no contagiousness to susceptible children, in tests to-date.
4. At least one vaccine appears to cause little or no excretion and it appears that non-excretion will be attained by 1967. Non-excretion precludes need for tests for contagiousness to pregnant women.
5. Scientific opinion and regulatory statutes differ concerning most desirable or acceptable kinds of cells for culture.

**Further work and forecast:**

1. Optimal vaccine remains to be developed and decided upon.
2. Safety and immunogenic capacity must be established in at least 10,000 susceptible persons with accepted vaccine strain.
3. Noncontagiousness or nonexcretion must be established absolutely

4. Protection by vaccination against natural rubella must be demonstrated definitively.
5. Research progress suggests that the acceptable virus strain may be developed by 1967 or 1968.

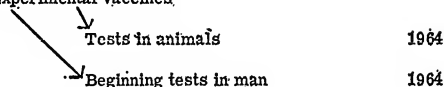
Pilot production work has already started commercially.

A vaccine might be available for routine use by 1969 or 1970.

#### (B) Killed virus vaccines

- Objectives:
1. Induce solid immunity for at least 9 months
  2. Safety to pregnant mother and to fetus

Experimental vaccines



#### 1. Special problem

The virus can be readily grown in acceptable cell culture and converted to a killed virus vaccine but the amount of virus is so small as not to be sufficient to stimulate an adequate immune response.

2. One killed vaccine, even 1000 times concentrated (economically impossible); failed to provide adequate antibody response or protection in man.

Reported vaccine "successes" not confirmed.



3. Outlook for killed virus vaccine is very poor unless some breakthrough in virus growth be attained.

The development of a safe and noncontagious live virus vaccine, used properly, can largely obviate need for a killed virus vaccine.

#### 5. Gamma globulin (purified human rubella antibody) approach

Gamma globulin has been used empirically for many years to reduce the chance for fetal infection and damage in pregnant women exposed to rubella, especially in the first trimester.

Cell culture (1963) permitted demonstration of rubella antibody in gamma globulin and selection of highly potent lots for human use.

Value of gamma globulin prophylaxis remains in dispute though perhaps a majority would agree that some degree of protection is afforded to both mother and fetus.

The danger of gamma globulin administration in exposed pregnant women is that of masking the clinical disease without protecting the fetus and thereby obscuring the need for alternative approaches that might be applied.

The way in which Buescher established the identity of the rubella virus is relevant to the general problem of the development of knowledge about fields of applied importance in medicine. Dr. Hilleman, who is himself a major contributor to the rubella breakthrough, has described it as follows:

Finally, the key piece in Buescher's work was the application of the interference phenomenon for virus detection. The interference phenomenon is many years old but was first applied in 1960 to detection of noncytopathic common cold viruses, now known as rhinoviruses, by Tyrrell in England. Buescher was doing a mass study to isolate viruses of this sort from undiagnosed cases of respiratory illness. In the course of specimen collection, some of the cases sampled were early rubella and these were not kicked out of the survey. The lucky thing was their inclusion since, on the first round, rubella virus was found. Buescher and his co-workers were real bright, realized the importance of the work, and gave the world rubella. (4)

Attention should be given to the fact that the original objective of the work leading to the discovery was something entirely other than the investigation of rubella. The perspicacity of the investigator in recognizing an important finding was the important ingredient.

Another example of the actual course of events in making major practical advances may be taken from surgery. Open-heart surgery was theoretically possible for a quarter of a century before it became a practicable procedure in treatment of human disease. At least five develop-

ments in other fields were essential to its realization. One was the totally unrelated development of a substitute for rubber, which could be used in tubing and which has less damaging effects on blood. Another was the study and development of methods for arresting or fibrillating the heart under conditions of successful reversal at will. Another was the development of minimally traumatic methods of artificially oxygenating blood. Finally, cardiac catheterization in man had to be made possible before diagnostic verification of intracardiac defects could be made prior to surgery. Still other developments unrelated to the technical surgical problem itself contributed to practical success. Some of these essential background developments came because of interest in the applied problem, but others were entirely unrelated as to original motivation.

### Serendipity in Medical Research

Rarely does a research project have a single objective, and rarely do two investigators approach a problem in identical ways. Consequently, the by-products of the research on a given subject will be different because of the different approaches. And often, the unexpected by-product turns out to be as important (or more so) than the original goal. Penicillin was discovered by Fleming as a by-product of other studies. The entire antibiotic development, one of the major medical developments of the last quarter century, may therefore be said to have arisen as a result of grasping the by-product of a study whose original objective was much less significant from a practical viewpoint.

This is no isolated instance. A case history of a development now in progress in my own department is illustrative of how this sort of thing happens. A scientist colleague needed to biopsy ventricular muscle from dogs' hearts to study enzymatic activity in relation to coronary blood flow and oxygen delivery. In isolated hearts this presented no problem, but with normally innervated hearts *in situ*, ventricular fibrillation regularly ensued, threatening to block the study. He investigated and rather quickly discovered a drug capable of preventing the fibrillation because he had just observed that the innervation of the heart played a crucial role in the disorder. He therefore had an important clue to start with and he was intellectually prepared to follow up that clue. It now seems that at least a partial answer to the problems of ventricular fibrillation in certain clinical conditions is at hand. The impetus to discover the useful agent was not initially the objective. Rather, a fortunate set of observations in another basic-research program suggested the solution.

The 1966 Nobel Award to Dr. Charles Huggins, for studies on hormonal control of cancer, is a richly deserved one. It may be pointed out, however, that the original observation on the basis of which hormonal influences on the development of cancer was discovered was an unpre-

dicted one, not directed at elucidating that problem; namely, that in certain strains of mice multiparous females had more breast cancer than did their virgin sisters. This led to an analysis of specific mechanisms and a demonstration of effects of estrogenic hormones in the process.

In the biological sciences there is so much unexplored territory in which basic information is completely or nearly absent that any hope of making important rapid strides toward solving the major practical problems such as arteriosclerosis, cancer, or mental disease *simply* by systematic applications of, or even directed development from, existing knowledge seems to be illusory.

### The Problem of Priorities

There may easily be a misconception with regard to the relative human value of different sorts of practical results in medical research. There might as a consequence be distortions in priorities. An example may make this clear. Arteriosclerosis in all its forms and consequences is today the largest basic cause of disability and death in the United States. A few of its complications we now know how to treat. We can today, for example, remove a thrombus, or remove and replace a segment of diseased artery. We can reduce arterial blood pressure by drugs to lower risks of rupture. We can sometimes resuscitate hearts in ventricular fibrillation. But prevention or reversal of the disease process is not yet in sight, and consequently, we work at partial solutions to consequences of the disease.

We might decide at this time to set up a very large program to construct an implantable permanent artificial heart to replace ones irreparably damaged by consequences of arteriosclerosis. We have, as a matter of fact, moved in this direction. There is no assurance that it can be accomplished, but it is certainly not possible to say that it cannot be done. There are many problems remaining to be solved. A more completely non-thrombogenic tubing must be devised. A suitable power system and a reliable non-traumatic pump of suitable dimensions must be devised. A new cybernetic system must be devised to be incorporated in the device. And unless structural breakdowns can be obviated over long periods of continuous operation the bearer of the implant will be as subject to sudden death as a most coronary-disease-prone person would be. Furthermore, only a limited number of heart-disease patients would be in the right place at the right time to receive and benefit from the device, if it were developed, unless it eventuated that the device were to be found to be so satisfactory in every regard that it would be proper to install it at virtually the first sign of disease, as far as use in coronary disease patients is concerned.

Despite such limitations in likelihood of success, and despite the limited usefulness that such a device would probably have, it is certainly proper

to attempt to build it. The question, however, is how much priority does it deserve compared with research on the prevention or cure of arteriosclerosis itself. Only a minor fraction of arteriosclerotics could in all likelihood be benefited by an artificial heart even if it were successful. The human value of a method or methods to prevent or cure arteriosclerosis would obviously be greater than the value to man of even a successful device that would only correct one of the consequences of the basic disease in a fraction of persons suffering from it.

The point to be made is that orders of priority must be respected, and both the probabilities of success and the magnitude of the human problems to be corrected must be taken into account in establishing priorities.

### **Some Present and Impending Impediments to Progress in Applied Research in Medicine**

A major complaint of investigators today is that an unwarranted and increasingly large fraction of their time and effort must go into scientifically unproductive work related to administrative matters. Specifically, for example, they question the utility of the so-called effort reporting stipulations currently required of grantees of the National Institutes of Health. An "effort report" gives rise to entirely meaningless numbers when a scientist is engaged in research on a general field, supported by two or more grants or agencies. This is especially awkward when his salary comes from one agency and his program support from one or more others. He cannot say, as he should, that he spends 100 percent time on the job he is personally remunerated for, and zero percent on the grant-in-aid of his program of research, without in many instances jeopardizing his program support.

It may be of some interest to the members of the Congress to reconsider particular directives it has given to Executive Agencies regarding the fiscal control of appropriated funds, particularly for grants in aid of research. Obviously, there should be no fraud in the use of any public moneys and safeguards should be set up against any misuse of public funds. However, patently good intent has resulted in some fiscal practices that are self-defeating as well as unnecessary.

A specific instance may best illustrate the problem. Associate Professor M. B. at University M. is appointed full time. He is expected to carry on both research and teaching. He spends 90 percent of his time at research, and that fraction of his salary comes from a Program-Project Grant from the National Heart Institute to the university. The remainder comes from university funds. No part of his research laboratory expense is paid by the National Heart Institute grant. He has a grant from another Federal agency supporting his laboratory work (but not his salary). According to present rules, he must report his "effort"

as 90 percent on the NHI grant that pays his salary. He does 10 percent teaching, so he must in all honesty report that to the university. It leaves him with no "effort" at all to credit to the research grant that pays for his laboratory expenses. If he reports zero percent "effort" to be put on his research project, he will get no support for it. No accounting solution is possible in this case. It is an extreme example of a class of cases of which there are many variants. The fiscal accounting gymnastics in which universities must engage in order to avoid illegal, but perfectly proper, actions are frequently not only absurd but also extremely costly. The "busy work" entailed involves not only clerks and accountants but also academic staff at all levels, and costs not only money but also the time of scientists, which is thus diverted from productive pursuits.

On the score of effort-reporting, it may be noted further that some of the new regulations actually encourage inefficiency and waste directly. A single university department with 20 academic staff members will ordinarily have at least 20 separate Federal research grants. In normal practice such a group will have a pool of five or six secretarial, clerical, and bookkeeping staff members to share the work for the entire group. It is an impossibility to predict in advance what share of each clerk's time will have to be spent on each research project in any given quarter of the year, yet this is what is now required. The tendency will be to add unneeded personnel to each separate project to make reporting easier. These may appear to be minor difficulties, but when many little frustrations are added up, the sum may be impressive. It may even drive some competent persons out of research careers.

In the February 1967 issue of the *Journal of Medical Education*, Dean Thomas B. Turner of the Johns Hopkins University Medical School discusses the same problems. After describing the difficulties outlined above, he says:

At first blush my remarks might be dismissed as petulant irritation over minor matters, but I caution against any such comfortable interpretation. Perhaps each item in itself is relatively minor, but collectively they constitute a large burden to the medical schools. One wonders whether the natural history of Federal assistance programs is one in which the grand conception and the simple inception by men of vision are destined to be followed by imposition of restrictions step by step to the point of frustration.

It is to be hoped that the sentiments expressed by the Bureau of the Budget in March 1966 in a report entitled, *The Administration of Government Supported Research at Universities*, may be implemented. A quotation indicates its tenor: "It is to the interest of both parties (the Government and the universities) to avoid over-administration of federally supported research. Too much or the wrong kind of control impairs the environment productive of good research. Over-administration could be more costly in diminished quality of research than in money

save by restrictions. Further, the cost of administering detailed regulations may exceed the losses from an occasional case of mismanagement."

The process of application of science to medicine is greatly complicated by the absolute necessity to make the final studies in every case on human subjects. In bald terms, this means human experimentation, because, before a new drug or a new procedure of any sort is tested on humans, there cannot be absolute assurance that results obtained on lower animals will be duplicated. The code of ethics of the medical profession requires that a physician obtain the informed consent of sick patients or of normal human subjects before he proceeds with any scientific study upon them, and obviously such matters as testing new drugs constitute such studies. The Congress quite properly has passed laws that protect the public, in so far as it is possible to do so without damaging the public interest by preventing progress against the use of potentially dangerous new drugs without their having given informed consent. However, a law itself is one thing, but its administration may be quite out of line with anticipation. In the United States we have an elaborate bureau dealing with the regulation of sale and human use of foods, drugs, and cosmetics. This Bureau—the Food and Drug Administration—has the power, subject only to court action, to prevent or stop the testing of any new or experimental drug. The Food and Drug Administration has been assigned a burden that should not be placed solely upon any Government bureau—of deciding whether to take the risk of letting thousands of persons suffer or die because it refuses to permit the testing of a potentially life-saving drug, or to risk the injury or death of a small number who would be involved in its testing—even after they were informed as to the possible hazards. To place such decisions solely on the shoulders—and consciences—of a group of Government employees who are open to the most drastic criticism and even dismissal for errors of commission, is to set the stage for a purely negativistic approach. And this has already happened. It is always personally safer to err by omission than by commission as far as criticism and job security are concerned. Certain other countries are meeting this problem by the use of external committees of the outstanding experts in the field. Then, if any errors do crop up, the prestigious committee of well-recognized experts is responsible, and not the Government officials.

It is no secret that, in general, Government regulatory agencies find great difficulty in attracting the highest scientific talent to their staffs. This is a fact of life and should be recognized by the Congress in the legislation that it writes. The persons they do recruit have in general a very high sense of public responsibility that would undoubtedly welcome the active help of the recognized experts in particular fields if the responsibility to the public of those experts were spelled out in the law.

If the Congress wants optimal progress in applied science in medicine, directed by the most talented experts, it should simplify the control of the use of investigational drugs by making more use of the most knowledgeable experts. No more money is needed. In fact, economies could probably be achieved.

Another hazard, still potential, to the advance of applied science in medicine is in connection with the possibility that the Congress may pass restrictive legislation with regard to the use of animals in medical research. The passage of bills like S. 1071 or H.R. 10049, introduced into the Eighty-Ninth Congress, would probably do more to cripple applied research in medicine than would cutting present appropriations in half. Almost all directly applicable medical research that is not performed on human subjects involves the use of animals that are intended to survive after surgical or other procedures that sometimes involve pain. If the Congress should unwisely pass legislation impeding such work, at the instigation of persons who are more concerned about lower animals than with human pain and suffering, great harm would be done to the public welfare. Hopefully, this will not happen, but Congressmen who are attempting to promote rapid advance in application of science in medicine should be forewarned as to this potentiality.

### **Problems of Management and Planning in Medical Research—Basic and Applied**

John Kenneth Galbraith, one of the most persuasive exponents of economic and social planning, has recently said: "It is a commonplace of modern technology that there is a high measure of certainty that problems have solutions before there is knowledge of how they are to be solved. It is reasonably certain that a man can be landed on the moon within the next five years. However, many of the technical details of this journey remain to be worked out. . . . If methods of performing the specified task have been fully worked out, it follows that the need for bringing organized intelligence to bear on the task will be less than if the methods are still uncertain. . . . Time and outlay will be even greater when methods are unknown or uncertain and where, accordingly, there must be expenditure for research and development. In these circumstances planning is even more essential. It is also more demanding. The time that is involved, the money that is at risk, and the number of things that can go wrong and the magnitude of the possible ensuing disaster all increase." (5)

Several points in connection with Galbraith's argument should be emphasized, and some questioned. In his first sentence he says that "problems have solutions" but he does not say that all problems do. It might have been better if he had said "many problems." Of course, the

"measure of certainty" for any particular practical problem depends upon the state of scientific knowledge about the problem. At the present time, for example, few if any experts in the field would be so bold as to assert that the cancer problem could be solved in five years, no matter what fraction of scientific and technological manpower available to us were to be put to work at it, even in the most highly organized and sophisticated way. Most experts would probably agree that some smaller segments of the problem can and will be solved.

There is a difference between attacking a practical problem involving research and development on "methods" when the more fundamental basic science is known, and an apparently comparable attack on a definable problem when basic phenomena underlying it are not known or understood. In the case Galbraith mentions—landing a man on the moon—at least as far as biological aspects are concerned the basic principles are sufficiently well understood so that biologists expect few real surprises in the performance of the man himself. We already know a great deal about his oxygen, water, and nutritional needs. We know about oxygen poisoning and how to avoid it. We know the effects of acceleration in takeoff, and enough about weightlessness to be in a position to be reasonably sure that we can deal with it. We know enough about effects of ionizing radiation, temperature extremes, biological rhythms, and the other relevant factors so that it seems that little further basic research will be necessary to make a trip to the moon feasible for a man. He is therefore justified in his estimate of probability, as to the biological side, because there is a large body of basic knowledge.

Galbraith is also correct that when methods are unknown the costs will be greater, but what he does not say is that the "measure of certainty" of success diminishes with the extent of ignorance, and that the money and effort at risk are more likely to be wasted. The real trouble with the Galbraith thesis, when even the nature of facts essential to a practical solution to a problem is unknown, is that planning in a meaningful and purposeful way is impossible, unless by planning one means investigation of all conceivable avenues of approach.

The proper strategy and tactics for basic scientific advances in medicine are not the same as those for applied technological advances, although they are intertwined. The best long-term strategy and plan for basic scientific advance in medicine appears to be to promote the education of highly talented persons and to provide them with the stimulus and the means to follow their noses, so to speak, in scientific inquiry. Their curiosity, the satisfactions of discovery of the secrets of nature, and the rewards of peer approval have in the past and undoubtedly will in the future provide the necessary stimuli to accomplishment. The role of management in this scheme is to decide in some way on how much money to invest in this or that area of basic medical science and to find means of



deciding which scientists should be given what amounts of money to work with. It must also decide on how much money to spend on education and training, and where. The last is a very important management issue, because it involves questions of the time span of the plan and of the relationship of scientific competence in various geographic population groups to industrial and cultural development in such groups. Thus, even though management does not attempt to regulate the precise directions of study of individual basic scientists, it does and must involve itself in the underlying question of how much support shall be given and under what kinds of directives as to time scale, including determinations about fractions of funds to be put into long-term educational as compared with short-term research-accomplishment objectives. There is in other words a major management role in relation to basic medical science.

As to applied medical science, the strategy of management can be much closer to the Galbraith ideas. Once the feasibility of a desirable application has become evident to experts, it will be in the public interest to invest purposefully in the further research and the development necessary to bring it to practical use. The amount and rate of the investment will presumably be decided in relation to the urgency of the development. The mode of direction of the program constitutes more of a problem. Even after general feasibility has been established, there are still alternative routes that might be followed. The example of the poliomyelitis vaccine is a case in point. It would have been a mistake to have foreclosed either the inactivated or the attenuated live-vaccine approach to the solution after general feasibility had been established. It is generally true that when more than one avenue toward a practical solution of a practical problem is evident, it is unwise to put all one's eggs in one basket, so to speak.

The Galbraith phrase, "the need for bringing organized intelligence to bear on the task," in connection with difficult problems deserves careful consideration. The key word is "organized," because no one would question the obvious need to bring intelligence into the work. Organization of intelligence is multi-faceted. Every scientific society arranges periodic meetings to allow a kind of organized sharing of information and ideas. Scientific periodical publications serve as major media of communication and represent a highly organized mechanism for alerting other scientists to facts and ideas. Symposia and conferences on special topics are going on every day in different fields of medicine to allow more sharply focused, organized, intelligent attention to ways of solving particular problems. In other words, in medical sciences, both basic and applied, there is today no dearth of methods for voluntary organization of effort. Parenthetically, it may be noted that there has been much unwarranted criticism of the travel costs of such meetings.

This type of organized sharing of information and ideas is, however, probably not what Galbraith had in mind. Many persons—some in medical science itself—would like to see a system of more centrally directed research effort, focused on specific applied-research objectives of high priority. They would like to see a system in which a socially desired feasible objective was identified, an estimate of probable cost of achievement made, a commitment for support given, presumably by Congress, and a mechanism set up to encourage competent manpower to work on specific aspects of the problem. Some such system, if operated with actual control in the hands of experts, would be a desirable addition to present management methods. The wartime Office of Scientific Research and Development might be a useful model.

The social desirability of an objective must be judged by society somehow, but the realities of the possible gains must be known. The latter must be explained by experts who know about possible limitations in usefulness. Feasibility is a matter entirely for experts to determine on the basis of evaluation of the completeness of needed basic information. Probable cost can be assessed only by experts who can know what needs to be done. As to the ultimate research direction, obviously complete knowledge of the problem and understanding of the research process itself are required.

The Congress has a major role to play in expressing the degree of social desirability, evidenced in its willingness to make the necessary dollar investment. To do so it must not only inform itself as to limitations and probabilities of success, as already indicated, but also make judgments as to the relative social importance of one or another desirable goal in the use of available funds. The Congress or any other supporting agency can, of course, also set up guidelines or directives as to how decisions as to feasibility, appropriate monetary emphasis on one or another alternative approach, and mechanisms of direction are to be made.

There is no doubt that supporting agencies must interest themselves in all these matters. Differences of opinion arise, however, as to two matters: (1) how decisions as to matters heavily dependent upon scientific knowledge should be arrived at, and (2) what sorts of directives should be laid down as to mechanisms of research management. As to the first, it would appear that it is essential that the management policy makers should obtain a broad spectrum of the most objective advice possible and should not depend upon advice from small numbers of specially interested persons, regardless of how well-intentioned those persons may be. The more detailed the directives top management intends to give, the more imperative it becomes that all personal and "political" (in the narrow sense) interests be excluded from control.

The major practical advances in medicine in the past have come by recognition by the funding agencies' advisory boards of the practical potentialities inherent in certain basic discoveries and the subsequent

especially liberal funding of research programs that their expert boards deemed essential to exploitation of basic discoveries. This mechanism has not resulted in inordinate delays in achievement and it has had numerous advantages over the establishment of permanent special institutes to approach particular practical objectives in applied medicine. The most important advantage in supporting scientists who become specially interested in the solution of practical problems in their own institutions over the establishment of special institutes to work at single problems is that the first is likely to involve more competent scientists. Except in most unusual circumstances, such as those existing when the atomic bomb was being developed, institutions are generally unwilling to give long leaves of absence to scientists to allow them to work elsewhere, guaranteeing their posts and facilities when they return. And scientists are not likely to abandon stable positions for two-to-five-year stints elsewhere. It might be suggested that permanent posts be set up for them in the applied research institutes, but this would involve complications and limitations. The institutes themselves would have to be adaptable to changes in program when particular jobs were finished, in order to be economical and justifiable, and particular types of experts suitable for the first missions would very probably not be the most suitable ones to attack new and different problems. There would need to be continuity, at least of general field, in an institute in order to justify appointment of permanent staff if it is to be strictly an applied-research mechanism.

Universal experts do not exist in science. A special institute for attack in tandem on a variety of problems would necessarily be staffed with persons more competent in some fields than in others. It could not be maximally competent in all. In our diversified and individualized society, with many institutions engaged in scientific medicine, it is inevitable that special competence in particular problems will be widely spread. It is highly probable that the great success of science, basic and applied, in medicine as elsewhere, in this country can be attributed in large part to the fact that many virtually independent investigators, each working with knowledge about what others have done and are doing, have been free to use their own initiative and best wits to dig up new knowledge.

All this is not to say that there is no virtue in institutionalized research in biology and medicine, in which a single long-term objective is pursued. For example, in studies of the biology of aging there can be little doubt that accomplishment is greater with institutional than with personal commitment. This is, however, mainly because of the inevitably long-time scale of the study. The same would be true of any other long-time observational programs.

Special problems arise in connection with interdisciplinary research programs. In the medical sciences, many basic and applied problems call for more expertise in mathematics, physics, and chemistry than the biomedical investigator possesses. To take a seemingly simple example,

one can look at the problem of a non-traumatic pump for blood. Especially because blood contains cells that are inherently fragile, the most complex problems of fluid mechanics enter. Shear stresses in relation to wall-surface characteristics, turbulence, tumbling of cells in streamline flow in relation to wall-to-center velocity gradients, for example, require for useful analysis investigation by experts in fluid mechanics. The biomedical scientist might acquire, or even by chance might have, an adequate background to make such studies, but if he does not, the time required to acquire it would probably be exorbitant, if alternatively he could collaborate with someone already equipped to handle the theoretical and practical problems involved. Other examples could be cited. How can the problem of organization be solved? Superficially it might seem that the biomedical scientist, given adequate support, could simply go out and hire the collaboration. It is not that easy, however. Competent creative experts in fluid mechanics do not need to be hand-maidens for biomedical scientists. They might be—and actually some are—much interested in the problem, but they are not in general about to abandon careers in their own fields to become junior partners in biomedical engineering teams. They might be willing to be full partners, especially if they are assured freedom to follow their own bents in what they do with their time outside of that required for the agreed-upon collaboration. There are, of course, other types of problems that can be facilitated by interdisciplinary collaboration where great originality and creativity is not required and the less than top-notch scientist can be very useful. In such cases, the problem of recruitment is not so great. But when very high-level competence is required, the association must be a voluntary one, and monetary incentives are probably not of the highest importance. A more important factor, in the past at least, has been interest of the interdisciplinary collaborator on the problem itself.

### **Suggestions Regarding Management Functions in Medical Research**

At least three kinds of medical and health problems appear to require special organization, planning, and funding. They are: (1) problems for which basic facilities, equipment, and operating requirements are so expensive that it would be unwise or impossible to attack the problems within existing institutional frameworks; (2) problems requiring studies of such long duration that the commitment of an individual investigator to a program would represent a hazardous investment, and an organizational commitment is required; and (3) problems of obvious public importance, attacks on which have not been initiated in existing institutions on a scale commensurate with their priority of importance.

Examples of (1) already exist. The medical departments of the Atomic Energy Commission's national laboratories may be cited. These laboratories provide opportunity for study by competent investigators from other institutions as well as of the laboratory itself with equipment

too expensive for installation in each institution. In addition they carry on long-term studies of public importance under category (2) above, such as the chronic effects of ionizing radiation on human populations. In other words, categories (1) and (2) may be combined in some instances.

Examples of (2) have been alluded too. Many studies of physiological and pathological processes in aging in man and other animals can best be carried out systematically in laboratories or institutes charged specially with carrying them on. It should not, however, be supposed that maximal progress can be achieved if all support of research in a given field is limited to one or even a few such special-program institutions. The experience of the past demonstrates that important contributions, even in relation to practical advance, have frequently come out of laboratories not primarily oriented toward solution of a particular practical problem. Furthermore, in connection with attempts to achieve practical results, as in connection with possible control of pathological processes in aging, it must be recognized that much basic research needs to be done. For each applied field there is probably some optimal mix of focus research and broadcast support of smaller projects in related areas.

In connection with category (3) there is the greatest opportunity for new ventures. Perhaps the outstanding example of an underfinanced and underdeveloped public health problem today is the one already mentioned—accident-prevention and treatment of trauma. No other problem affecting life, health, and happiness to such a great extent has received so little research attention and support. A few medical and other scientists have devoted themselves to study of this problem, but it has not received the spontaneous attention that it deserves, perhaps because there has been a paucity of practicable ideas for solution. This problem will involve, even for its partial solution, the coordination of efforts of many kinds—educational, engineering design, public service supply (for example, if fire and police protection are appropriate public services, why are not highway ambulance services, accident patrols, and the like), medical and surgical treatment, hospital management and facilities, among others. Without foreclosing support of more limited-objective programs, it would seem that the public welfare demands that some agency capable of committing large sums of money, presumably the United States Congress, should consider seriously a major applied-research and education thrust at this problem. It could properly be assigned to a special institute with large high-priority support. It happens that committees of well-informed scientists have, as already noted, asked for consideration of such a program.

The foregoing discussion points the way to certain guidelines for the management of research capable of leading to desired practical objectives. First, it is axiomatic that nothing should be done to frustrate

creativity in either basic or applied research. Conversely, changes could be made to lighten the administrative complexities, ("red tape") or research support already provided. Likewise, the Congress should protect the ability of medical and other biological scientists to carry on their studies effectively.

Second, it seems desirable that there be better communication between medical scientists and the Congress with respect to the management function for medical research. The country has been extremely fortunate that, during the years when Federal support of medical research was developing, the U.S. Public Health Service, and particularly the National Institutes of Health, has had such wise leadership. But obviously the Congress is looking forward to new initiatives and deserves the advice of the best talent that country has to offer in helping it to appreciate technical information in exercising its proper management function. There is a challenge here in the medical science community itself, because at present there is no single body that satisfactorily represents medical science in a broad way in this country. The best existing approach to such a body is the Division of Medical Sciences of the National Research Council of the National Academy of Sciences, and it might be utilized more fully by the Congress.

Third, it is evident that whatever mechanism the Congress chooses to use to help it establish its policies and priorities, it should not neglect the consideration of longer-term objectives for the country as a whole, particularly with respect to support of facilities for and operation of educational activities in the development of research capabilities.

Finally, it must be emphasized most strongly that it would be a grievous mistake to think that all practical problems in medicine could be solved more expeditiously by centrally managed planning. Numerous examples have been given of instances in which practical progress was frustrated until basic observations made without reference to the particular applied research goal, had been made, after which the practical goal became relatively easy of achievement. The genius of science, apart from the enormous power of the method, lies in the intelligence, the training, and the enthusiasm of its practitioners. There is probably no area of human endeavor in which the "free-enterprise system" has been more successful than in science.

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## **SHAPING THE MIND: COMPUTERS IN EDUCATION**

*by* RALPH W. GERARD

### **Introduction**

Bernard Shaw once said, "The reasonable man adapts himself to the world; the unreasonable man attempts to adapt the world to himself. Therefore, all progress depends on the unreasonable man." Science and technology, like the unreasonable man, frequently, and often violently, move to change the world. Kepler and Galileo set man's static earth spinning through the skies and wrenched his theology irreparably, but this had little impact on the way he lived. The identification and then transmutation of elements stirred men's souls far less, but the successful manipulation of matter, as in metallurgy and synthetic chemistry, has revolutionized his way of living. A century ago, Darwin's insights again shook man's belief, this time in the domain of life; but applications to human evolution remain to the future.

The next great disturbance we can expect from science relates to mind and behavior and it will come from the application of behavioral science, mainly in education, more than from theoretical advances in it. The great computer systems that von Neumann sparked supply the technology that will give man control of the mind, as Pasteur's germs, and later antiseptics and antibiotics, gave control of infection, and as Mendel's breeding can control species. Every important scientific achievement has been either a basic understanding that toppled man's beliefs or an applied outcome that recreated his environment, or it partook of both. The impending upheaval in information handling, in ways of thinking and communication, in molding the mind and even the brain, in setting goals and shaping behavior, promises to be the greatest that science has triggered. Man will live very differently as a result of it. We must face up to many disturbing problems; not least the ethical one of mind-manipulation.

This essay concerns the technology of human behavior. It faces, first, the value problems involved in tinkering with man's mind; second, the biological approaches to altering behavior and the accompanying brain changes; and third, the impact of computer technology on the far-flung educational processes and establishment. In its closing, the longer vista of social consequences is glimpsed.

## Values

It is easy to see the limitations of the beliefs—superstitions we are likely to call them—of primitive peoples, but our own convictions also tend to be the more compelling as they are based more on tradition than on experience. As Brock Chisholm, a wise psychiatrist and first Director General of the World Health Organization, wrote: "For most people conscience is something that is not questionable—that gives an answer without thought—that is a feeling, which produces in relation to certain ideas, or certain forms of behavior a feeling of virtue or, on the other hand, a feeling of guilt or shame. For most people this voice, which is internal, is accepted as ultimate authority, their basic authority. It occurs to relatively few people that the language in which conscience speaks is for each of us entirely accidental. It is determined by the family in which we were brought up and by the attitudes which were about us when we were small; and largely its development is finished by about six or seven or possibly eight years of age" (1).

To make the issues that the new technology must raise crystal clear, I shall take a strong polar position in favor of activities usually denigrated by such terms as "brain-washing," "indoctrination," "invasion of privacy." These terms suggest "man's inhumanity to man"—the using of a person as an object to serve the selfish aims of the user. But not all aims are selfish; a surprising number of man-manipulators are highly altruistic. They operate for what they believe to be the best interests of their targets. The parent, the teacher, the preacher, the doctor, the adviser or guide in general, attempts to mold the person or direct his behavior; if honorable, this control is intended for the benefits of the controlled, if wise, it is in fact so.

### *Indoctrination*

The whole enculturation process is one of indoctrination—in this culture, to accept that the word for a furry, purry animal is "cat," spelled with three letters with certain sounds; that a head nod means "yes;" that  $2+2=4$ ; that every seventh day is special; that a lady is ushered through a door by a gentleman; that communism is bad and (mostly) that either the Democratic or the Republican party is good; that certain ways and values are preferable to others. Without such common acceptances a coherent society could not endure; indeed, with strong subcultures developing in which some of these are not shared, society is today under a severe strain. Yet none of these conventional assertions is an inborn or universal verity; what is inborn is a complex highly malleable system. Normal human babies can learn all these conventions; simian babies can not. Even the mathematical equation varies in its symbols, and all differ among cultures, even to the extent that an adult is quite unable to change. A Japanese, unless raised on our culture, never



really masters the "r" and "l" phonemes, nor can an American sit comfortably on his heels, as can an oriental. City children rarely overcome a fear or unease with farm animals, theists and atheists do not easily enter the world of the other, oriental bathing and toilet habits disturb the occidental, and few Americans enjoy fresh mealy worms or fried grasshoppers, as do many aborigines.

The problem is not with indoctrination as such, we cannot have a civilization without it; the problems are rather those of the means of indoctrination and of the control of these means by whom and for what ends. We think of the hucksters, the con men, the dictators, the zealots, even the bad parents, who use subtle or harsh means to bend others to their purposes, which we judge bad by definition (exploiting the object) or by our own values (dishonesty and force are unacceptable). Printing and publishing, radio and television, public address systems and mood music and tricky lighting, all extend the reach and effectiveness of indoctrinator or charismatic leader; increasing behavioral science, giving to the "menschenkenner" the added understanding of man's drives and emotions and how to play upon them more certainly, will similarly increase his power and control.

This is the universal problem of increasing science, of more powerful means to a wide spectrum of ends. Though many have been concerned at past achievements of science, and a few have actively opposed them, the bulk of mankind is eager to move from savagery to civilization, from the naked perilous inarticulate existence of "natural" man to the comfortable protected information-overflowing urban life of the "advanced" communities. Nor have men been able to reverse the direction of change; science pushes the groups that have embraced it on an exponential curve of autocatalysis. Education, as maker of the minds of men and vessel for the social transmission of all culture, as an accelerator of acceleration (a "jerk,"  $\frac{d^2}{dt^2}$ ), is doubly powerful—and dangerous.

### **Control**

As any system becomes more complex and integrated, the interdependence of the components increases and the influence of the whole on the parts does likewise. This is true for organisms and for societies. When we come to depend on mass supply of food and clothing, of power and transport and communication, we also become more dependent on the men and machines dealing with these commodities. A widespread airline strike or power failure is devastating to great cities, almost unnoticed on small farms. To the extent that parents have, by choice or law, stopped home tutoring of their young in favor of community schooling, they have relinquished control of their children's formal education, and to the extent that family groups have yielded to peer groups they have relinquished control of the total upbringing.

All this is not necessarily bad; much is excellent. Authors and teachers, with the aid of books, can offer an education beyond the reach of any solitary parent. Children of foreign parents or those of one native subculture can learn the ways of another culture only from contacts outside the home, though this process often creaks badly at present. The mass media certainly shape the development of a generation and perhaps, though this is freely questioned, more for better than for worse. Their control is in the hands of a few men who are committed by the rules of the game to a successful financial operation of these subsystems, whatever other goals are kept fresh; where profit is less crucial, other values should gain.

It is a biological truism that the organism more fitted to an environment is more likely to survive in it, and the social evolution of institutions is basically similar. Large systems can develop subspecializations and do more things more effectively (and economically) than can small ones, and great corporations continue to spring up and flourish in industrialized countries, whether aided or restricted by public policy. The worry, of course, is that control may become so centered in Government itself that a power elite can dictate to, rather than be instructed by, the citizenry. And control of information and of education is absolutely crucial in manipulating people's current and long-range attitudes—witness the high priority of broadcasting stations in a revolutionary coup. Partly for this reason, education in this country has been kept mostly in the hands of very local groups, with little support or direction from the Federal Government. This has permitted, indeed, great variation in the educational experience of our children; but the evidence suggests that much of this variation is in the substandard direction.

California, with a population of approximately 18.6 million and some 4.4 million children in its schools through grade 14, is larger than most nations. Expenditures run about \$50 over the national average annually per pupil and, despite great local differences within it, the State does set many educational norms, as by choosing textbooks. Such norms are not generally regarded as pernicious, nor need Federal standards be so. In France, long the cynosure of the cultured world and still muddling along without falling into either totalitarian extreme, a minister of education could say, "At this time of morning I can tell you that every third grade class in France is studying the subjunctive of the verb 'to be'" (2). This seems to us too great a regimentation, and indeed France has suffered in consequence; but when we look at the caricature of education presented too often in this country by fearful, ignorant, and often hostile teachers in shabby quarters and with almost non-existent resources to unwilling, scornful or openly rebellious youngsters from racially or economically underprivileged sources, we may be less fearful of the influence of Federal education.

Nor has big government made a bad record in education and education-related activities—witness the National Science Foundation's support of high school science curricula. The great granting agencies in Washington have contributed to our major universities so extensively that nearly all would collapse if Federal funds were sharply curtailed. Yet, while supplying the means—building, equipment, personnel—Federal officials have avoided prescribing programs. The university scientists of this country are so pleased with their direct support from Washington that many oppose any strengthening of the role of their own institution in the distribution of Federal resources; and not only the legislators and public but many in academia as well begin to suggest more instances of centrally planned programs. Moreover, the academic world has come widely to respect the competence and integrity of those administering the Government agencies in this area—from the early days of the Office of Naval Research right to the present. Why should they not? These men have come from and return to the academic ranks and they consult intensively with the men and institutions using the resources they dispense.

Actually, mass development of technologies of communication and, so, of education have led to the loosening of centralized control of men's minds. Printing and books, significantly starting with Gutenberg's Bible, are credited with the successful revolt of the people from tight clerical rule. The typewriter and telephone, possible only with massive conformity, have enormously increased the flow of individualized messages with, essentially, no constraint on their content. Videotape is rapidly reaching the stage of home (or school) production and use, with individual (or local) decision as to what is produced and presented. (Incidentally, home movies have long been with us but few of the local productions attain willing audiences even without charge, as compared with the mass-produced and often tactless professional ones for which people pay admission.) When the great computer systems and data banks and networks are in operation, there should be greater freedom of local content choice and even production than is presently the case with packaged books or tapes or movies. Teachers will continue to prepare the messages, however complex or universal the medium for their presentation.

### *Privacy*

The spectre of Big Brother in 1984 is real—and indeed “eternal vigilance is the price of liberty;” but technology can aid vigilance as well as power. It is feared that great data bases of information about people will provide dossiers to be misused by the unscrupulous (e.g., 3). This is a legitimate fear, but security of information is easier to maintain with a computer system than with present document files, and checks on human controllers of such systems are also possible with split authorities and with technology itself. We hear a great deal today about individual rights

and the infringement of privacy, but very little of the advantages of the inevitable relinquishment of certain privileges as men crowd into social living.<sup>1</sup> The battle of fences versus open range, between the sheep and the cowmen in the opening west, was a perfect paradigm of the problem of private versus public domain. Society did not hesitate to impose on individuals vaccination and quarantine, since an infected individual was a menace to others (at the subhuman level, herds of infected cattle, for example, have been ruthlessly slaughtered). It imposed schooling since an uncultured person was also a threat to others; it specified fire or driving or sanitary safety standards on the property of an owner for the protection of others. Society, dependent on customs and laws for needed conformity, applies informal or formal sanctions to transgressors. We find the gain possible from the social contract greater than the sacrifice it requires.

Yet there is now an outcry against asking or testing for certain information for census purposes, for schooling and placement evaluation, for taxing and credit, for legal and law-enforcement needs. Because information can be used unscrupulously, the old adages urging maximum information for the electorate and honesty as the best policy seem to be in eclipse. It is forgotten that in most cases where "prying" into the "affairs" of an individual is resented is because there are dirty corners that violate social or legal norms—granting that some "play" in the system is desirable. Our laws against self-incrimination are properly intended to protect against coercion, especially by physical force. Should they, however, really prevent a responsible law-enforcement officer from obtaining a drop of blood for an alcohol test of a driver involved in a traffic accident? Recognizing its high, though fallible, performance, why should not a lie-detector test be given and the results, expertly interpreted be used in reaching decisions? Why should a statement heard with the naked ear or an event seen with the unaided eye be legal evidence, but the same information gained with a microphone or an infrared scope be inadmissible? Is not the complaint rather with incompetent or criminal use rather than with the technology; and is not the answer improvement

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<sup>1</sup> As Ogburn wrote thirty years ago: "A very good illustration is one discussed in another chapter, that of securing useful information about an individual. In a stable community with aggregations of population little larger than a village an individual becomes generally quite well known even to his minute idiosyncrasies. The requirements of the task which he or she is sought to perform are also generally known. The situation is quite different in a complex society undergoing rapid change with large populations and a good deal of mobility. There is thus an urgent demand to know more about individuals. So there are psychological tests, school grades, fingerprints, lie detectors, case history records, vocational guidance agencies, etc. The influences of these various inventions all flow into the same groove leading toward more information about the person concerned. It is the social need that determines the groove" (7).

rather than abolition—which is clearly impossible, especially against those flouting rather than supporting the rules of society?

A full and thoughtful treatment of the technical, psychological, social, and legal aspects of the ancient and modern problem of privacy, disclosure, and surveillance has recently been published (4). The conflict of rights between individual and group is clearly recognized (see also 5, 6, 8), and the lag in legal attention to the progressive invasion of privacy by technology from 1880 to 1950 is documented. Westin (4) points out the importance of official blinking at violation of generally rejected laws, as those against gambling or prostitution, and justly states (p. 1046): "Only those who can sustain an absolute commitment to the ideal of perfection can survive total surveillance. This is not the condition of men in ordinary society." Clearly, an alternate remedy in such cases is a more realistic body of law, both as to what is proscribed in individual behavior and in the investigation of individual behavior. Many feel today that snooping has gotten out of control and wish to return to the level of privacy possible in the 18th century. Since the integrative forces of society inevitably increase as does interdependence, this is unrealistic. Surely abuses can be checked, but what is regarded as crucially private is also changing. Full candor is today socially intolerable, but candor is seemingly increasing in human relations—witness responses on sex, finances, beliefs not only to anonymous pollsters but also to acquaintances in psychotherapy or sensitivity-training groups, now so popular—and might conceivably obtain even in diplomacy. At least a study of foreign officers in our Department of State suggests that greater candor would be desirable (9). We may indeed be moving toward "preventive mental health."

What is needed is a less confused information flow and clarification of its acceptable uses. In any event, technology in the behavioral sciences can be used with increasing power and reliability to reveal unacceptable attitudes and acts of individuals. Or acceptable ones; the use of a reverse lie-detector situation has been suggested, to allow a nation's leader to prove to other nations by "truth demonstration" that his public statements are indeed sincere and not diplomatic bluffing (10). And technology can offer countermeasures, as jamming, to illicit surveillance.

The above treatment is far too brief an entry into a great and complex, as well as emotion-laden and legally bristling, field. It is intended to challenge contemporary attitudes and so promote objective study—not to disprove them by further rhetoric.

### Brain Changes

This extensive foray into value problems was essential before examining the promise, or threat, of the new knowledge and technology that are upon us in the area of brain and behavior. Every advance opens clear

possibilities for application that impinge upon strong beliefs or emotions. My primary concern in this essay is the enormous impact on education, and so on all facets of our life, that new information-handling technology will make possible; but, perhaps partially for desensitization, a look first at more direct ways of affecting the nervous system is in order.

The brain is in essence a giant network of billions of nerve cells, or neurones, of many types, each possessing long, hairlike extensions to connect with many others. Electrochemical changes sweep along these connections to activate or quiet neurones, which in turn increase or decrease their own activity and the pulses they emit. The environment, by playing upon sense organs, initiates patterns of input messages along sensory nerves that specifically represent the situations that triggered them off. These sensory inputs activate particular groups of neurones, which continue to send further messages to and fro, with timed and repetitive signals often reverberating along selected channels for long periods before fading out, or discharging along motor nerves to produce muscle movements and other kinds of behavior. One might think of an outside signal which starts the church bells of a village chiming, and then the bells leading one another into and out of a melodic ringing of changes.

Since neurones are living and highly labile in state, a given sensory input does not always evoke an identical internal activity pattern or an invariable behavioral response. Just as muscles perform better after a warming-up period, show fatigue after maintained activity, and hypertrophy (a kind of learning) with repeated use, so neurones respond more or less easily depending on their own past activity and improve their performance on repeated experience. Far more than muscles or many other organs, nerve cells are highly sensitive to small electric currents, to a variety of chemicals (as drugs, hormones, toxins, neurohumors, and other normal or foreign substances) and to changes in blood supply or its contained nourishment. All these, as well as past experience, can greatly alter the amount and kind of activity set in motion by various external situations.

### ***Biological Control***

Many potent drugs are now available for many effects on behavior; how they affect the brain so as to alter behavior is rarely clear. Sedatives act to decrease activity, excitants to increase it; most drugs have mixed and complex actions. Yet even a "sedative" barbiturate given to solitary mice, puts them to sleep; given to mice in "social" groups, often rouses them—much as alcohol does to man. Narcotics also have multiple effects, as do tranquilizers and the various "psychedelics" coming into widespread and dangerous use. Many drugs have long-range effects, especially after repeated use, associated with habituation and addiction; a few

can produce enduring change after a single dose. Psychotic and neurotic patients today are regularly given treatment by tranquilizers and related drugs, and this group has become, after antibiotics and drugs affecting blood pressure, the most widely used pharmaceutical—one sixth of all prescriptions. In 1964, 140 million doses (costing half a billion dollars) were dispensed in response to legitimate medical prescriptions. A large illicit traffic, moreover, exists in these chemicals as well as in marijuana, LSD, "pep pills," the morphine group, and other drugs used by physicians little or not at all. Here, clearly, are powerful ways of controlling behavior by direct action on the brain.

Behavior can be also specifically manipulated by applying electric currents or drugs to chosen regions of the brain. This requires, mostly, the placement during an operation of fine, stimulating wires, electrodes, or tubes, chemtrodcs, and so is less immediately at hand than the usual use of drugs; but procedures are relatively simple and *could* be applied widely, and the effects are dramatic indeed. Stimulating one small region in the base of the brain will make an animal drink lethal quantities of water; stimulating a nearby one will cause it to thirst to death. Other centers control eating or running or fighting or sex or other activities. A house cat with the appropriate part of the brain activated attacks ferociously; a wildcat, with this region suppressed or destroyed, becomes tame. Animals will forego food and sleep for days to press a lever that delivers stimuli to "pleasure" centers in their brains—probably the same ones that, stimulated in humans, lead to strong sex-related sensations. Still other regions evoke in man sensations of vision or other senses, or cause past experiences to unfold as vivid memories, or initiate complex hallucinations or other abnormal experience or behavior. Here, also, is a wide avenue to control of the mind.

Many other agents can be applied to the body to affect behavior: sex hormones that masculinize or feminize, thyroid that heightens activity, adrenalin that generates anxiety, strychnine that raises irritability, and many others. Mention might be made here, though the underlying basis is presented later, of claims for drugs or brain extracts that enhance learning or memory. Much research is in progress supporting or, mostly, denying the possibility that extracts (supposedly of RNA) from brains of rats that have learned a particular performance, injected into the brains of naive animals, decrease their learning time for this performance. One set of experiments, for example, claimed to show that extracts from rats taught to turn right in a T maze hasten the learning by injected rats to make a right turn, slow the learning of a left turn; and extracts from rats trained to turn left have the reverse effect. Confirmation is certainly required here. Hypnosis also needs mention, but cannot be adequately discussed; it is more related to learning as such.

While drug use is widespread, other manipulations mentioned above are likely to remain as laboratory or hospital curiosities for the foreseeable future. Somewhere intermediate is the control of brains by development. Controlled alteration of genetic materials by chemical and other agents is some time off, but there is no question that selective mating now could shape future generations of men as it has done for plants and animals. With the arrival of sperm banks and ovum banks, with improved means and growing acceptance of fertility control and birth regulation, with strong pressures for quantitative limitation of population now widely recognized and qualitative selection more possible, an effort at genetic guidance for improved "intelligence" or artistry or benevolence or flexibility for further evolution (surely desirable), or what you will, may not be far off. The ethical question of tampering with human evolution is spurious; we are already doing so strongly. Modern medicine, public health, social welfare are decisively altering the reproductive balance between various gene pools—and not toward enhancement of the species. It is not too early to give serious thought to preferred directions of guided reproduction, for the means are upon us and almost surely will be applied, wisely or foolishly.

### ***Behavioral Control***

When messages pass from one neurone to another there is a small easing or enhancement of their future passage over that particular connection. Frequent repeated activity thus, like a rivulet deepening its bed, channels certain easy paths for itself. When habits form, behavior is conditioned, experience is internalized, there are real and enduring changes in the nervous system. Baby chimps, raised with no opportunity to see patterns for a couple of months, are unable to discriminate objects when later given the opportunity. They are functionally blind; the absence of normal visual exercise during the early development of the brain led to defective connections dealing with visual messages. Recently this defect has been directly demonstrated, not only in brain structure but also in its functioning. Microelectrodes have been put into or near individual neurones in the visual cortex of kittens, directly measuring the traffic of nerve messages in and out. Normally, a tiny spot of light on corresponding points of the right and left retinas starts messages in nerve fibers that reach the same few neurones in the cortex. Mostly, such neurones are stimulated to send on their own messages about equally well by those arriving from either eye. If, however, one eye is kept covered for five weeks after birth and then uncovered, messages from light run up to the cortex in the normal manner but fail to activate the cortical neurones on reaching them. And this defect persists; the covered eye, or rather the brain it serves, remains blind. In the same way a baby with "cross eyes," so that light from a given object doesn't fall on equivalent retinal points



and the baby sees double, comes to neglect the messages from one of the eyes. Unless corrected early in life, the "suppressed eye" becomes blind.

Many other studies point the same way and even have demonstrated structural and chemical changes associated with activity that relate to the functional changes just mentioned. The brains of highly inbred strains of rats are remarkably uniform, so that consistent differences found between groups kept under one condition or another can be highly significant. In series of tests, some rats grew up in an "impoverished" environment—one to a cage, no "toys" or handling, unchanging environment with minimal sound or light; others were in an "enriched" environment—playmates and play objects, much petting and other benefits. The cerebral cortex averaged about 5 percent thicker in the enriched group and some important chemicals were more abundant. (No great extrapolation is required to see why children reaching first grade from environments in which the written word is nonexistent and the spoken word almost equally so are disastrously handicapped for their schooling experience, or to appreciate the great potential of remedial pre-school programs such as "Head Start!") Some of the chemical changes found in the brain in relation to learning involve the production of more or different RNA molecules and of the proteins and enzymes these produce in turn; and many scientists associate memory with these particular substances—perhaps acting indirectly to facilitate the passage of messages from neurone to neurone. Hence the attempts to aid learning and memory (even in man) by RNA and comparable injections.

Whatever the biology involved, experience can strongly influence the brain. Not only does deprivation limit brain development, desirable activity enhances it. As training in sports or other physical activities markedly increases skills and performance, so appropriate exercise of the brain improves intellectual skills and performance. A good coach and a good teacher have comparable roles—to guide learning by well-chosen experiences. Psychiatrists and clinical psychologists have long offered controlled experience in psychotherapy of various sorts to aid the mental health of adults and children. No one knows what improvement in performance could be reached by an average child given better learning experiences, but competent judges have guessed several fold! At least, I.Q. differences of 10 to 20 points or more have been reported as induced by environmental conditions. The answers, of course, will be reached by really examining and experimenting with the total education experience and carefully studying the outcomes. Skinner (11) quotes Pascal: "Habit is a second nature which destroys the first. But what is the nature? Why is habit not natural? I am very much afraid that nature is itself only a first habit as habit is a second nature." Since so much of present education is mortally ill, there is little danger of

harm in careful experimenting, and vast promise of good. And at last the technological means of adding science to art in education are available.

### Technology and Education

Learning is the modification of behavior by experience; and formal education is a conscious effort to modify behavior in chosen ways by appropriately structuring experience. This is indoctrination, however done; it might as well be done with some elegance. Elegance has mostly not been the case. Goals beyond the most elementary rote learning of the R's, have rarely been clear; learning theory, or even heuristics, have been almost nonexistent; educational procedures, when happily good, have been so because of gifted individual teachers; leadership and thoughtful innovation have been rare and often frowned upon by the community; perhaps most depressing has been the almost total absence of self-examination of the process of education or evaluation of the product. From education technology must come an educational science to supplement its art.

With school people given low pay and, often, regarded as lackeys of parents or town fathers; with the swelling needs for teachers met all too often from the less motivated and able; with slender resources, crowded classes, and heavy administrative overload; with students moving in a solid phalanx through a uniform curriculum of fixed content and length for each class—although differing abilities and achievement from subject to subject often found grouped in the same activities children with an effective range of seven years or more; with even the best-willed elementary teachers devoting, on the average, under one seventh of pupil-contact time to actual teaching, and this divided among 20 to 40 or more children, of whom progressively more in present schools resent being there; with all these adverse factors it is perhaps more remarkable that schooling, like Samuel Johnson's dog, can stand on its hind legs than that it does so poorly.

This country has always been convinced, as part of its democratic heritage, of the importance of widespread education; and it has been convinced, no less, that this should be kept in local control. As masses grew and mass schooling became reality, however, local resources could not keep up and today the small community, unless an exceptionally wealthy one, is unable or unwilling to buy quality education. Like the impoverished buyer in general, these pay more money and receive inferior goods. As former Commissioner of Education, Francis Keppel, has emphasized (12), quantity rose and quality fell.

The case of instructional television is illuminating. This clearly valuable resource has more or less failed in this country, while it has been highly successful in many others (both advanced, as in France, or re-

tarded, as in American Samoa),<sup>1</sup> where it has been developed on a nationwide base (2). Local units here have proven too lacking in resources of money and skill to turn out even creditable products; and truly superior ones are essential even to win a fair test in the face of teacher conservatism, let alone to do a better educational job. The science high school curricula supported by the National Science Foundation, requiring resources not locally available, are an example of a nationwide effort that has had considerable success in this country.

In the horse-and-buggy days, local artisans could supply the individualized needs of their townfolk; when technology presented the gasoline buggy, the great nationwide automobile industry soon sprang up and offerings became more standardized. Yet who would surrender the super-highway and easy transportation (yes, even in rush hours) for the dust, mud, and manure and the clop-clopping trips of the past—except in moments of nostalgia. More people are able to get about on their individual errands since road transportation became mechanized and centralized than was remotely possible with individualized resources. More standardized resources thus permit more individualized uses. (Much the same point is made in the report *Public Television*, just released by the Carnegie Commission (13)). New problems have, of course, arisen; few qualified judges doubt that further science and technology, if given a chance, will resolve them.

The point is made in Weinberg's essay in this volume that social problems that resist social solutions may yield happily to new technology. This may well prove the case for education. The blackboard, the slide projector, the television screen, even the book (at least in early stages of learning) are at best aids to the teacher, who remains the focus of the pupil's learning. When programmed instruction made a stumbling entry, even when the vastly greater potential of computer programming appeared, the emphasis remained on the teacher—witness the terms *teaching* machines and computer-aided *instruction*: But, of course, the learner, not the teacher, is the proper center of focus; the teacher—except as he helps set the goals of education, which occurs essentially only at the highest reaches of the university—is just a learning aid to the student. Educators do prepare curriculum content and teachers do give their charges more than policing and drill; they offer important emotional interactions,

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<sup>1</sup> President Johnson stated in a release, November 26, 1966: "During my recent trip to the Far East, I visited the educational television station in Pago Pago, American Samoa, and saw how television is being used to improve the level of learning in elementary and secondary schools."

"I believe that educational television can play a vital role in assisting less-developed countries in their educational effort. These stations can be used for adult education and information programs during evening hours. Community leaders can use these channels for discussion of important public issues."

especially in lower grades, and intellectual interactions, especially at higher levels. But the former duties tend to overwhelm the latter and, if technology could take over the lesser jobs, fewer and more gifted humans would be able to do better the more creative jobs. Computer systems have reached a stage where rote drill, individualized tutoring, even Socratic dialogue is possible between machine and user; when such systems become widely used the teacher will no longer be essential in most phases of the actual learning process—indeed, the organized school itself may undergo metamorphoses or disappear.

Humans will always have to set the aims of education and to create the materials and sequence them for the learning experience so as to approach these goals. Humans will also always have to interact as humans with the young; infants, animals or man, given full physical care but kept in biological and psychological isolation fare badly, if they survive at all. But different humans may well serve the separate needs, via existing or different institutions, and most of the direct interactive learning experience, especially at early levels, can then be presented by a computer-tutor rather than a human robot.

The present computer—and new generations continue to arrive every 2.5 to 3 years—can already handle dozens of terminals at the same time, responding promptly (under a second) and individually to the specific needs and performance at each user. The terminal hardware is now so convenient, and the special languages required for man-machine communication so near natural language, that a completely naive user can be guided by the system itself to proficiency in use and mastery of a chosen subject, without human aid except in emergencies. Computer-aided *learning*, CAL, is rapidly penetrating schools, exploring with special vocational and professional groups, and flirting with total communities. Although hardware must and will improve in performance and, even more, in cost characteristics, the real problems today are in software, in content material and, to a lesser extent, programs for computer performance; and in organization for effective use. Involved here are: curriculum building; arrangements for rewarding the creation and dissemination of CAL materials; developing comprehensive shared data banks; establishing compatible and convenient information networks; exploring the problems of widespread cooperation of institutions and teachers in producing and using educational materials; productively integrating books (via microform), sound records, movies, video materials, any recorded form of man's collective experience and creations, into computer-mobilized resources. Included also are: better understanding of the learning process and learning how best to aid learning—when to instruct or drill or examine or answer or discuss with the student; devising and using satisfactory criteria and measures of the behavioral gains that education is supposed to be furthering. Many of such needs

are not new, nor are they unique to CAL; what is new is a dawning science that makes it possible to formulate incisive and meaningful problems in the field of education and reasonably to expect that solid answers will be forthcoming.

### **CAL**

In order to build a course, or a shorter instructional block, for CAL, the objectives of each exercise must be distinct, the reasoning sharp, the logic sound, and the facts and responses correct. An hour's interaction may require one or two hundred hours to prepare; but in the course of this, involving preliminary testing on students, fuzzy or dull or otherwise inadequate portions should have been corrected so that a really incisive educational instrument is the result. This can then be used with unlimited numbers of students, located anywhere—at homes, in other parts of the country—at any time and with any amount of repetition. Further improvement and updating is as simple as typing a few lines; and many alternative presentations will surely be developed as the scale of use permits. The limited experience to date indicates that students cover material (skills and understanding as well as knowledge) faster (three to five times have been reported) than in ordinary classes, achieving greater mastery, with better retention, and with great satisfaction. At college level, more complex and instructive problems can be assigned. Children and adults, alike, are led on as in a game and often, if permitted, work at a course hours on end—somewhat reminiscent of a rat stimulating a “reward” center in its brain by pressing a lever, or a pigeon similarly “earning” grains of corn.

The computer can keep a record of the full interaction with the student, with two, perhaps three, major gains. For the educator, the microrecord of performance allows easy and pinpointed experimentation with alternate ways of presenting material of various sorts to various kinds of students, thereby rapidly improving understanding and practice in education. For the student, a record of what he has learned and his particular learning idiosyncrasies can govern the heuristics (rules of thumb) used by his “tutor”—whether the computer responds more actively or draws out the student, whether it tells or asks, whether ideas are best presented first by example or introduced at once as general principles, whether small steps and repetition or great mental strides are needed, whether visual or auditory presentation is most helpful, and so on and on. It bears reminder that two children of equal intelligence may vary a thousand-fold in the ratio of one particular ability to another. Little of this is yet being done, but many sparks are lit and there is no question of technological (if not yet economic) feasibility. The third gain of the performance record is that, at the end of a block, the student has in fact demonstrated mastery and has passed his examination; the computer, thus, can teach and independently certify achievement.

Many other gains should follow. With highly individualized instruction, curricular units can be made smaller and combined, like standardized Meccano parts, into a great variety of particular programs custom-made for each learner. Unplanned repetition and waste, greatly present in higher level courses at least, should be eliminated, and content much better tailored to the intellectual-shape and goals of each user. The opportunity for a student to learn pretty much at his own convenience of time, place, pace, and procedure is hard to over-value and, although the need to gather at a school may in time disappear, two or more students could easily work together at a terminal (some studies suggest this is effective as well as economical), and two-way or conference-type interactions with teacher or total "seminar" groups are also entirely possible. The altered roles of teachers have already been mentioned; where the teacher does remain in the immediate classroom situation, relief from routine chores should be most helpful. Finally, with spatial gathering minimally necessary, and educational demands and the means to them more widely interchangeable, the single school and classroom and promotion might well give way to regional or state or national educational systems and graduations, or other certifications of achievement—as in the British "University of the Air."

It would take us too far afield to examine present efforts in CAL; many, notably one at Stanford, are described in recent publications (14). At the university level, the Interuniversity Communication Council, EDUCOM, is rapidly bringing the institutions of higher learning of this country into a working information network; and the larger state systems, as the University of California or the State University of New York, are developing supporting INTRACOM systems. I may be permitted, however, a passing reference to the present use of CAL at the University of California; and to a Lifetime Learning Center, "an ambitious attempt to consider the community as a total information system, within which all the functions of teaching and learning are carried on in both formal and informal settings," in the planning stage with the University of California at Irvine, the Irvine Company, and the General Learning Corporation as cooperating participants. (Parenthetically, the important and related developments in the uses of computer systems in administrative matters at schools, for generating true systems planning by these institutions, in research, to aid hospital operations and medical practice, and particularly, still within the educational arena, to mobilize library documents and eventually to deal with the contained matter directly, all omitted here, deserve separate attention.)

The University of California at Irvine has been committed to full utilization of computers from its inception. By the time faculty began to arrive, two years ago, a cooperative agreement with International Business Machines made available a moderately powerful instrument, housed

and staffed in trailers at interim quarters, and half a dozen on-line terminals were active. Now, part way through our second academic year, 18 terminals are kept active 70 hours a week and batch processing is carried on at off-hours. Course enrollment has been computerized from our opening and academic records and reports are handled with this aid. Some accounting is automated and more administrative and library functions are being programmed. CAL has been used by about half of our 1,600 lower-division students, for short or long times, in such course work as: introductory psychology, economics, biology, information and computer science; and in remedial mathematics, English, history and Government. A modified and a new computer language have been developed for computation and they are widely used by advanced students and faculty in their research. Well over half the faculty is using a computer in research and about a fourth are concerned with CAL, many actively engaged in programming course content. A new system, now being installed, will soon permit the greatly increased use that is clearly demanded.

### **Costs**

In the mid-sixties education has become this Nation's largest industry. Many figures in Keppel's recent book (12) support this statement. Half a century ago, the average schooling was through eight grades of elementary school, today it is well over the 12 grades through high school. Some 50 million pupils are today in elementary and high schools and the number is increasing by one million a year; over three fourths of our population 17 years old or over graduates from high school. A total of 123,000 schools and 2.4 million teachers are teaching 55 million students at a yearly cost of \$39 billion.

In 1962 the Federal expenditures for education were \$1.8 billion (2 percent of the total Federal budget); states' expenditures were \$10.7 (34 percent of their total budgets); local expenditures were \$17.9 (45 percent). The total public and private outlay for schooling (1963 estimated) was \$35.9 billion. (A recent article (15) gives \$48.8 billion as the figure for 1966 and adds \$12 billion for educational activities in industry.) Total education costs (\$3.5 billion) were 1.8 percent of the gross national product in 1943; 3.8 percent (\$14 billion) in 1953; and 6.1 percent (\$36 billion) in 1963. The average cost (constant dollars) per pupil-year in all public schools rose from \$106 at the end of the thirties, to \$259 in one decade and \$472 in two; it now stands at \$641. Some 350,000 classrooms were built in the first part of this decade and nearly as many more will be needed (even at 30 pupils in a class) during the last half; and the bonded indebtedness for buildings rose \$13 billion in a decade, to \$17.5 in 1961-1962—with over half a billion dollars interest per year.

Expenditures are far from uniform between states or districts in a state. In 1959-1960, the cost per classroom ranged from \$3,645 (Arkansas)

to \$12,215 (New York); in 1965–1966 teachers' salaries varied from \$4,190 (Mississippi) to \$8,240 (Alaska); and the cost per child-year ranged in New York alone from \$300 to \$2,000 (1962–1963) and from state to state from \$249 to \$749. By 1965–1966, Office of Education Title I funds were giving significant aid to state budgets—from 15.0 percent (\$31 million) of Mississippi's to 2.3 percent (\$78 million) of California's. The rapid growth of funds available (from \$0.12 billion in 1956, \$0.35 in 1961, to \$1.5 in 1966; with a total O.E. appropriation for 1966 of \$3 and one of about \$5 recently passed Congress for 1967) will surely lead to a general upgrading of education and especially to intensive experimentation involving new technologies.

The computer and related industries have burgeoned in the past decade and give every evidence of increasing growth. An informal guess (16) is that the 1,000 computers in this country in 1955 will have risen to 70,000 by 1975. They will be 1,000 times as powerful as now by 1980, completing  $10^9$  operations per second and at  $1/200$ th the present cost. Large computer memories of  $10^{12}$  bits are now at hand, each able to handle  $10^{-8}$  of the information (non-redundant) in all libraries of the world. The Federal Government alone spent \$840 million from June 1965 to June 1966, running 1,800 computers—mainly for fiscal use.

Transistors and related solid-state developments became commercial only about half a dozen years ago and gave the decrease in size and power requirements and increase in reliability needed to touch off the computer age. With miniaturization, thin film, and integrated circuits, costs have dropped by a factor of  $10^3$ , and circuitry costs first, now memory costs, are becoming relatively unimportant. An estimate (17) shows a modern computing system (32 K magnetic core memory) now costing \$1.5 million to make will soon cost less than one fourth as much. To be sure, as the logic demands rise the load on the computer goes up rapidly, especially when used in an interactive on-line mode as in CAL—the number of branches rises precipitously with extension of moves in a chess game—but simple drill and tutoring is less demanding and, if highly idiosyncratic responses of students are handled indirectly (e.g., 1 percent are asked for a different response or referred to a teacher), the branching is greatly decreased. In any event, for CAL at elementary levels with multiple access systems in effective use, costs per terminal hour are already below \$1—and children being taught from scratch to read English in 200 hours (18).<sup>1</sup> (See also references 19 and 20 to the effect that rehabilitation and vocational training can be accomplished at a total cost per student of less than \$200, at under \$1 per hour.)

Some have estimated the cost of preparing CAL materials at \$4,000–\$10,000 per hour on the computer. This seems excessive, even if 100–

<sup>1</sup> This initial estimate has later been revised upward. It probably remains valid if hardware is amortized over 10 years rather than rented.



200 instructor hours are required, plus some programmer and machine time; but even if a figure, say, of \$5,000 were accepted, the cost is small compared to hardware cost if materials are widely used. A single hour-T.V. program can easily cost 50 fold more and yet be profitable; if a CAL program were used in only 1,000 classes one time it would be entirely feasible. Actually programs could be used more widely and over many repetitions—with easy updating every year or two. I estimated in 1965 that 100 lower-division college courses, which should cover present offerings quite well, could be put on CAL for \$3 million; even if this estimate is low by a factor of ten, the cost would still be small. In the lower schools, with more restricted curriculum options, with even greater gains in learning time possible for CAL hours compared to usual class hours, with more pupils and simpler machine demands, CAL should show a great pay-off, both in cheaper and in better education. (The Carnegie Commission incidentally, recommends about a quarter of a billion dollars annually for adequate "Public Television" and something comparable for "Educational Television.")

How soon and how extensively the usual classroom teacher and the usual classroom can be shunted by CAL remains to be seen. If the present school personnel and material (including buildings) were cut to one tenth, some \$50 billion a year would soon be released. Assuming a school year of 1,000 class hours for the average pupil, and \$1 per hour per terminal, CAL would cost \$2,000 per pupil year—some half again the present national average—or \$100 billion per year. This limited increase of present costs (at most temporary with improving technology) neglects, however, the more rapid learning with CAL. Even with a halving of learning time (let alone the 3–5 fold reported), CAL would today be considerably less expensive than conventional education. (This neglects other kinds of learning, as social or motor skills or attitudes and motivations which occupy a significant portion of present school hours.) If the present education through high school were dehydrated from twelve years to nine, as is the case in several European countries, the decreased teaching costs and the gain in learning power would yield an annual saving estimated (21) at \$15 billion.

### ***Organization***

Smaller school districts are unifying into larger ones and minimal standards are being pressed by the states. The U.S. Office of Education is tooling up to handle the great increase in Federal funds, which will surely help spread and upgrade education. Industry has recognized the opportunity presented by the metamorphosis of education from its prolonged horse-and-buggy era to a technologically modern activity, and is stirring itself mightily. Companies expert in computers, in communication, and in publication and editing have been associating to create independent

or subsidiary entities. In the past year over 120 such hybrids have been formed (15), of which the best known include RCA and Random House, IBM and Science Research Associates, G.E. and Time, Inc. (to form General Learning Corporation), Raytheon and D.C. Heath & Company and others. These, in turn, are establishing ties with educators and schools and universities; and all are looking to Federal and other governing agencies for cooperation and support—much as in the aerospace field.

Actually, a new kind of utility is coming into being, with education and entertainment (hopefully more related than in the past) and banking and other information services destined to enter each home via appropriate wall plugs and terminal equipment. The Federal Government, especially Congress, will be faced with major problems of control, support, and cooperation. Information is the commodity and will be more difficult to handle than objects. When computer programs and data banks, more than particular books or films or tapes or discs, are the crux of the matter, copyright laws, as one example, and property rights between the Government, business, schools and scholars for another, demand a complete overhauling. Despite Marshall McLuhan, the *message* is our concern, not the medium; and our brains and behaviors respond to the meaning rather than the avenue by which this is received. Much creative study, and top-level interaction between Government, industry, and education, are urgently needed to effectively channel the flooding opportunities.

### Technology and Society

That the new information-processing technology will profoundly alter man's ways is widely agreed; whether for better or worse is debated. There certainly is a danger of damage by new ingredients in our social nexus. I would call this "social toxicity." Urbanization, made possible by advances in the technologies of housing and transportation and hygiene, in particular, certainly have carried unanticipated elements of social toxicity that have fostered delinquency and crime and alienation. Perhaps, with communication about to leap-frog over transportation, urban sprawl will decentralize to community spread and so solve many problems. The new ones must then be faced in turn.

An anthropologist left a steel machete with an Andean Indian tribe that had no metal tools, and almost exterminated them: it enabled them to collect large quantities of cane to ferment for an annual "get it off your chest" fiesta and, instead of patching matters up in a mildly euphoric mood, they got into lethal drunken fights. Outcomes cannot be fully predicted, although a growing and sturdier behavioral science will improve prediction. My own conviction is that the new will bring far more gain than loss.

Neglecting all else, few men would exchange the human estate, with all its enhanced problems and suffering, for the simian one. Yet there

is reason to believe that man's great cerebrum, which makes man human, itself evolved from a technological advance—the use of hand tools. The new information technology, involving both CAL in educating our youth and a computer-man symbiosis in problem-solving by our adults, might well propel man into a new species. Certainly man must upgrade himself to keep ahead of his automata, and certainly he will be doing very different things soon if one thoughtful analysis (17) is correct: "By 1972, allowing one to six years to design and build automated production systems, a large majority of the Nation's jobs now in existence will be obsolete." And certainly CAL offers hope of breaking through our present educational impasse that helps maintain an underprivileged and progressively unassimilable part of our society. It may be the solution to "the contradiction in practice between quality of education and equality of educational opportunity" (12).

Nor have the anti-technological prophets of doom mostly been right; social toxicity has rarely been correctly predicted, the troubles have not been those feared. When trains were coming into use in the 19th century, physiological disaster from moving the body at 20 miles an hour was predicted; when documents appeared in antiquity a decay of memory was forecast that would "produce only a race of imbeciles," a view supported even by Socrates. Change is always disturbing and education, accelerating change, has been an enduring problem to society. A difficulty has indeed been that, "the society which education served has in general been defined by what it has been, rather than what it might be" (22).

Social evolution was superposed on biological evolution when a brain capable of learning and transmitting that learning came into being and made a collective culture possible. Then man interacted with his environment more in terms of information flow relative to energy and substance flow; and, having fairly solved the physical and biological problems of control of his material environment, he now lives in a sea of information that he has created and to which he must adjust. I see the main epochs of social evolution as: the use of symbols; the organized use of symbols, language; the tested organized use of symbols, science; the extrasomatic manipulation of symbols, computers. This technology of information-processing, channeled into education, will be fully as important to mankind as is language or science. Science, like the unreasonable man, will re-tailor the world to man; yet in an unexpected way, will re-tailor man as part of progress.

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## THE ENVIRONMENTAL SCIENCES AND NATIONAL GOALS

by PRESTON E. CLOUD, Jr., and V. E. MCKELVEY

Civilization is the product of the interaction of man with his environment and its resources. Man's efforts to understand and utilize the products and forces of nature for his own ends gave rise to science and technology. In turn, their advance has vastly strengthened his ability to manage and exploit the natural environment. In exploiting nature, however, man may affect it irreversibly, and in unforeseen ways that limit his subsequent use of it, or that may impair the quality of life which entire communities, regions, and generations may lead. His technology, moreover, does not sufficiently take into account the nature and availability of raw materials, the consequences of their processing and use, or the needs and aspirations of the people it serves.

Such questions involve the environmental sciences, by which we mean those integrating sciences, such as ecology, geology, oceanography, and meteorology, that are concerned with the natural environment apart from the societal component. We may then designate as environmental engineering the application of environmental science for the benefit of man—for example, in agriculture, forestry, sanitary engineering, public health, and the engineering phases of geology, oceanography and meteorology.

By necessity, as well as by tradition, environmental science and engineering in some form have always been special concerns of Government, and must continue to be so, for the simple reason that they affect the people in inescapable ways that transcend all private activities. Private industry can be involved to a limited extent by appropriate appeals, rewards, and penalties, and the universities, as conservers and innovators of knowledge and culture, will continue to train environmental scientists and therefore to maintain a modest research effort. Only Federal- or State-supported organizations and laboratories, however, can be expected to undertake the comprehensive, long-continuing, and costly regional, nationwide, and oceanwide assessments and repeated surveys necessary to understand, to utilize effectively, and to live in harmony with our changing environment and its resources.

In view of this special concern of the Federal Government with the environmental sciences, we find it germane to describe here some of their

distinctive features and to discuss their application and management, with particular regard to their bearing on the attainment of national goals.

### Nature and Relationships of the Environmental Sciences

Certain characteristics of the environmental sciences bear critically on the feasibility of particular goals, and on research strategy and management. As integrating sciences, they apply the primary sciences—physics, chemistry, mathematics, and parts of biology—to study of the earth, its lithosphere, hydrosphere, atmosphere, and biosphere. In such studies they display conceptual developments similar to the primary sciences—observation, hypotheses to relate and explain observed phenomena, and verification to test the validity of both observations and hypotheses. In their concern with the real natural environment, however, they deal not only with the inherent properties and configurations of phenomena on a regional or worldwide scale, but also with configurational change through time, that is with a historical sequence of interrelated events (1). In this sense they have no close operational parallel in the primary sciences except on a micro-scale. The physicist, for example, is interested in the inherent properties of the atom or the crystal, and the chemist is concerned with reactions that take place over relatively short intervals of time. But in studying the inherent, more or less time-independent, properties of phenomena observed, and in seeking to develop general truths about these properties, physicists and chemists are not much concerned with the broader natural settings in which materials or phenomena studied may occur, nor with their historical antecedents or causal relationships.

Primary science may, indeed in most cases must, set the conditions of the experiment. Its results, therefore, are characteristically quantitative, cumulative, and simplifying. Environmental science may and frequently does perform simplifying quantitative experiments, but eventually, and mainly, it must deal with the real, excessively complicated and often very large-scale interacting processes and results of nature's experiments. Its results, therefore, are often qualitative, distributive, and seemingly complicating. Nevertheless, as does all science, it seeks to formulate testable simplifying models, and when it has done so these often have great strength because of the large number of variables which must be consistent with one another and which therefore give independent checks on the validity of the model.

Because of its concern with the broad configurations of nature and their development through time, environmental science also tends to have a distinctive regional and historical focus. It seeks to explore, characterize, and understand local and regional similarities and differences in geologic, biologic, oceanographic, atmospheric, and pedologic (soils science) phenomena, and to comprehend both the evolutionary develop-

ment of such phenomena and their likely interactions with the activities of man.

In view of this regional and historical focus of the environmental sciences, regional surveys and time-referenced research are fundamental to their development and application. Such operations are the prime means of identifying, and to an important extent solving, problems in the environmental sciences. They are also essential to defining the nature of the local and regional environment, to preparing for its wise use, and to providing the basis for confronting intelligently a wide variety of often unforeseen problems that arise from human contact with the physical world. The applicability of environmental science to any specific goal, therefore, depends not only on the complexity of the problem, but to a large extent both on the pool of available relevant knowledge and on the insights of available operators.

Examples of this linkage between regional studies, insightful operators, and other aspects of science and society are legion. A striking illustration of the feedback between survey and theory is provided by recent surveys of rock magnetism. Although rocks showing magnetic reversal have been known since 1906, it was not known whether this was due to some special property of the rock or to actual reversal of the earth's magnetic field until regional surveys revealed formerly widespread episodes of reversed magnetism. Even more startling is the unpredicted fact that volcanic rocks at the sea bottom show regionally extensive north-south striped patterns of magnetic anomalies that are remarkably like the reversal patterns observed in time sequences. These magnetic belts are symmetrically arrayed on both sides of and parallel to the mid-ocean ridge in at least one region, and at other places they are offset by east-west topographic trends. Such regional observations strongly support the theory of sea-floor spreading as a result of rising convection cells under the mid-ocean ridges (2, 3) as well as the existence of large east-west zones of sea-floor displacement by faulting. What practical applications this may have are less clear, but it could well have a bearing on submarine navigation, the employment of magnetic mines, and on earthquake and tidal wave prediction and control. It is worth noting that nothing in the primary sciences would have led one to look for such features, but that physics, a primary science, was essential in their study. Once revealed by regional surveys, moreover, these features suggest new concepts in the physics of earth-magnetism.

Regional studies have paid enormous dividends to the economy of this Nation from the very beginning of its history. Among many specific examples is that of a study of the East Tennessee Zinc District made just before World War II by the U.S. Geological Survey. Here, geologist Josiah Bridge, studying and mapping local rock distribution as a part of a larger regional survey, discovered that the eastern limit of the producing

mines was a low-angle thrust fault that brought a relatively thin slice of older unproductive rocks up over the ore-bearing rocks. His suggestion that new ore bodies might be obtained by exploring beneath the barren sheet of overthrust rocks was tested by a mining company. As a result, large quantities of an accessible but concealed strategic metal were found and the economy of a region was benefited. More recently, combined gravity and geologic mapping in the Los Angeles basin (4) has led directly to the discovery of four new oil fields with a total value estimated at about \$250 million. In another example, recent detection, as a result of regional geologic mapping and synthesis of trend lines seemingly controlling mineralization in Nevada (5) has defined target areas for further mapping and for exploration by industry that may lead to amelioration of current heavy metal shortages. Indeed, at least one new mine, the Carlin Mine, has already gone into operation in what appears to be a major new concealed gold deposit as a result of this work (Ref. 5, pp. 60-61) backed up by refined analytical techniques, such as atomic absorption, which only became available a few years ago. Regional mapping of trace elements in plants has, on its part, led to advantageous changes in grazing patterns and even to discovery of ore deposits. And regional studies of the Great Lakes fisheries, *after* the disastrous introduction of the lamprey, appear to be bringing about lamprey control and a new fisheries system that, if not as good as the \$5 million a year industry destroyed, will at least permit a partial recovery.

Similar examples could be multiplied for all of the environmental sciences, often involving circumstances where an application was unforeseen, sometimes representing deliberate intensive effort toward a practical end by an agency, a laboratory, or an individual. Thus our present weather warning system and expectations for weather control are also the outgrowth of regional and synoptic surveys. So too is much of our magnificent agriculture. And so should be our future use of the earth either for conservation and recreation, production of food and fiber, exploitation for industrial materials, the quelling of pollution, or the avoidance or amelioration of catastrophe.

Regional surveys have no close parallel in the primary sciences, but in time and cost they are major activities of the environmental sciences, and they are never-ending. Even mapping is a never-ending activity, for the progress of science and technology makes all maps out of date after a decade or two, whether or not there are actual changes in the surface mapped. It does this on the one hand by making it possible, through the development of new methods and concepts, to record significant phenomena not differentiable before, and on the other hand by introducing needs for kinds of information or at scales of detail not required before. In preparing for sensible long-range planning, much more extensive regional surveys than are now being carried out need to be made (and



repeated)—both by men on the ground and by remote sensing devices from the air and on and under the sea. For neither use nor conservation can be practiced wisely without clear knowledge of basic regional characteristics and requirements.

The complexity of natural phenomena gives rise to other distinguishing features and problems of the environmental sciences that are of concern in Federal planning. The interaction of climatic, oceanographic, geologic, and biologic processes, for example, is reflected not only in the difficulty of understanding or even satisfactorily characterizing many natural phenomena, but also in the effects of human activities. The environment and its component resources represent an interlocking, dynamic set of systems—a super-system if you like—in which man himself, in using it and seeking control over it, has become its dominant element. Wittingly we use and modify the environment and exploit its resources. But unwittingly and in ways that we do not always anticipate or understand we are profoundly affecting the system, in many ways adversely for future purposes. Few would have imagined, for instance, that flood control on inland streams might have adverse effects on the coastal environment. Yet recent surveys demonstrate that this is exactly what is in the making for California, where beach sands in constant long-shore flow and loss via submarine canyons depend for their continuance mainly on a regular new sand supply from inland streams whose sediments are now being trapped behind flood-control dams. The consequences of cutting off this sand supply, unless counteracted, will be far reaching, not only for recreational use of the beaches, but for coastal life and fisheries as well.

The environmental sciences, even in their current stage of development, could and should be used more fully than they are in solving practical problems. It is also true that they can be and need to be much more advanced in a fundamental sense in order to contribute in full measure to the needs of society.

Environmental engineering also needs to be better developed and more widely utilized. It has wide and economically significant application even in non-quantitative terms. The simple avoidance of or accommodation to sites of potential damage or danger in location and design of buildings and public works, for instance, is still by no means widely enough practiced. Millions of dollars worth of damage and lawsuits in the Los Angeles area testify to the fact that construction has taken place at sites that should have been reserved for parks and that highways have not been properly designed for the terrain traversed. Yet many of these sites were clearly outlined as ones of potential earth movement on inexpensive published maps that pre-date construction. The landslide of 17 August 1959 near Hebgen Lake, Montana, was more disastrous than it might have been because public campgrounds had been located directly beneath an area which a trained eye could easily see was one of recent and im-

pending large landslide activity. The dust bowl of the 1930's was at least in part attributable to the dry-farming of lands that should not have been intensively cultivated. The use of heavy farm machinery in some areas is reducing the capability of the ground there to hold and transmit water. And, even in recent years, relatively new farms in dry areas of our Western States have had to be abandoned because they depended on the "mining" of water beyond its natural rate of replenishment. Proper land use in the tributary uplands in other areas would concurrently conserve water and reduce need for flood control in the lowlands.

Other examples of the need for more and better environmental science and engineering abound. The reindeer herd of Alaska dropped from about 650,000 to 25,000 in about ten years, and the caribou correspondingly, because of failure to understand the ecology and carrying capacity of the foliose lichens on which they grazed (6). Commercial fisheries, such as for clams and oysters off New England and sardines off California, have been adversely or catastrophically affected by unforeseen ecological disturbances, and many more such effects can be expected in all sectors of the environment as our choices in its management are limited by a variety of controllable as well as uncontrollable events. All utilization or exploitation of natural resources by man, to be sure, involves some hazard and risk of damage to other resources. We do not suggest, therefore, that risks not be taken or environment never disturbed, but we wish to emphasize that better understanding of natural phenomena and processes, and adequate dispersal of protected natural environments would make it possible to reduce risks, minimize damage, and preserve the potentiality of restoration.

The interdependence among all sciences is an important factor in developing research strategy and tactics. The environmental sciences, to be sure, provide an indispensable input to the mining, water resource utilization, sanitary engineering, agriculture, silviculture, wildlife management, fishing, outdoor recreation, construction, chemical, manufacturing, transportation, and communication industries. The primary sciences, however, are the source of knowledge from whence arises the technology by means of which we use the environment and its resources for human purposes. The primary sciences and associated technologies, in fact, create the demand for natural materials by determining which are "resources," and they also create the machines and methods for developing, processing, and transporting such resources. Similarly, it is the primary sciences and their derivative technologies that will provide the means to control pollution and perhaps weather. But in developing the tools and methods that make it possible to exploit the environment and to manage it constructively, the primary sciences and technologies, in turn, function most effectively in alliance with a deep understanding of natural phenomena, processes, and materials—in interaction with the

environmental sciences. Design, for instance, might call for the use of a rare or metallurgically intractable material where a less well known but commoner or more amenable material would suffice. In order to choose the best, most abundant, and most tractable materials, as well as those whose recovery, processing, and use creates the fewest problems, comprehensive censuses of the distribution of both real and potential resources are needed.

The environmental and primary sciences thus have complementary roles in environmental exploitation and management, and together they provide the means to utilize our environment wisely. Dynamic balance and imaginative flexibility are the keys to an effective Federal policy for science and technology, with due recognition that a wise balance involves response to changing needs, conditions, and opportunities.

Somehow all of these aspects of the environmental sciences must be taken into consideration in any well organized national plan. We do not propose here to present such a plan but only to discuss some factors that bear on its formulation.

### **Bringing Environmental Science to Bear on National Goals**

#### ***General Strategy***

National goals may be classed broadly as maintaining the security of the Nation, advancing the level of living of the people, improving health, and adding to the amenity of life. These carry to more specific goals, some of which may call for a readily prescribed program of action, while others may represent intangible ends to be worked toward over the years in diverse and flexible ways. The establishment of goals at this level is a matter of policy, here taken as given. We respond to the question: What bearing do the environmental sciences and engineering have on their attainment?

Most national goals are long-continuing, so much so as to make it germane to examine both short-term and long-term strategies for achieving them. It is safe to say that all involve the environment in some way. They often also contain inherently conflicting elements. Energy is a good example. National objectives are essentially to maintain an adequate and secure supply that is dependable under emergency conditions, available at reasonable cost, and produced and used in ways that cause the least possible damage to health or environment. For security reasons, it may be judged necessary to maintain domestic sources of production. Then, if domestic sources are more costly than foreign sources, subsidy or other protective measures that add to cost may be required. And regulatory measures to prevent or control environmental pollution or other hazards to health or resources resulting from winning, extracting, or utilizing energy resources may similarly add to cost, or may make it

difficult to produce or use energy in ways that are best from the standpoint of national security and economy.

A comprehensive strategy must, therefore, not only weigh and balance means but also effects in both the short term and the long term. In settling on strategy, it may be necessary to compromise, or to choose one course for the short term while developing another for the long term.

Although it is safe to assume that research and the application of science can contribute to the attainment of any national goal, science and technology are by no means the only instruments for achieving national objectives. The first questions, then, in formulating strategy are: (1) What are the alternative methods, both short-term and long-term, for achieving the goal? (2) What are their comparative costs, as well as other advantages and disadvantages? (3) What would be their respective probable effects on the achievement of other national goals? (4) What would be the consequences to the national well-being and costs to the national economy of doing nothing?

Answers to these questions for any given objective would most likely lead to a strategy that employs several different methods in complementary roles, particularly over time. Regulatory methods, for example, may be most effective in the short term, whereas science and engineering are likely to provide more effective long-term solutions. And, most important, science and engineering provide the best means of simultaneously achieving objectives that are or appear to be conflicting. For example, regulations governing pollution abatement inevitably add to manufacturing and perhaps consumer costs, but development of means to recover noxious pollutants profitably has the potential for reducing costs while eliminating the hazard.

### *Identifying Practicable Objectives and Soluble Problems*

Selecting specific objectives that are ripe for intensive programmatic research requires coordinated input not only from competent and imaginative scientists and engineers but often also from other interested citizens. Preliminary research may be required to explore feasibility, cost, and side-effects before it can be decided whether or not a concerted research program would be worthwhile, and the range of choices may be much influenced by the stage of development of the underlying science or even by historical accident.

Means to minimize or avert the disastrous effects of severe earthquakes, for example, have traditionally been restricted to research in structural design, backed by building-code regulations. Recent analysis, however, of a time-limited swarm of small to moderate earthquakes in the Denver area strongly suggests that they resulted from the deep, high-pressure injection of fluid wastes along a complex natural fracture system; and this opens up the possibility that the crustal strain responsible for some earthquakes may be relieved gradually and far less disruptively (7, 8).

It is instructive to consider more specifically how this unforeseen possibility came about. Involved in it was an unprecedented incidence of earthquakes in the Denver region from April 1962 onward, a chemical-waste-disposal program of the Rocky Mountain Arsenal, a coincidental relation between deep-injection disposal wells and geological structure, geologists of the U.S. Geological Survey, an alert and public-spirited independent consulting geologist (David M. Evans), and an equally alert and perceptive Congressman (then Representative Roy McVicker of Colorado). Insight began with Evans' hypothesis, based on available predictive theory of fluid pressure in relation to earth movement (9), that there might be a relation between the high-pressure injection of wastes, a multiple-fracture zone, and the earthquakes. After this conjecture was locally publicized, Representative McVicker was instrumental in obtaining intensification of a Geological Survey study of the problem. That investigation virtually demonstrated a casual relation between fluid pressure in the injection wells, fracture zone, and earthquakes. In itself this was of immediate practical importance only in terms of allaying local concern. It has led, however, to an appreciation of how some fault structures might be made to move a little at a time to release as minor earth tremors stresses whose unrelieved accumulation could eventually result in destructive earthquakes. Of course, this does not solve the earthquake problem. Rather it is a new liberating concept that identifies the problem of earthquake control or amelioration as being ripe for concentrated attack. The U.S. Geological Survey, following the recommendations of a study panel of the President's Science Advisory Committee, has now organized a special interdisciplinary research unit to mount such an attack—the National Center for Earthquake Research at Menlo Park, California.

The point of interest about the Denver earthquakes, is that they were in an area, and concerned a specific problem, where considerable information and theory had become available, and where investigation was already in progress and needed only the encouragement of sufficient funds to reach a local solution quickly. In the absence of suitable theory (developed, as it happens, from the data of regional surveys), qualified earth scientists, and sufficient useful facts, results would probably not have been forthcoming. And in the absence of these local results, the larger possibilities would probably not have been foreseen.

To be emphasized is the fact that the existence of a problem and the desire to do something about it in themselves carry no assurance of rapid success in the environmental sciences. It is necessary, therefore, to maintain a great breadth of competence and activity under circumstances of easy and open communication, so that the practical implications of new knowledge can be identified and applied. And, important though this is to all citizens, the rarity of clear relevance to particular private activities

means that the necessary knowledge and theory simply do not accrue at a favorable rate in the absence of a broad program of governmental support.

Recognition of this by national leaders accounts for the fact that there has always been fairly broad governmental support of the environmental sciences in this and many other countries, both nationally and regionally. It is because of the ever increasing numbers of people and their increasing demands on and rates of interaction with the environment that new and larger problems are constantly arising and that even broader and more intensive programs of environmental research and engineering are needed.

Pollution of air, water, and soil is, of course, presently a conspicuous environmental problem, and certainly a critical one. Our very existence, as well as our good health, depends on adequate supplies of pure water, clean air, and productive soil. Congress is well aware of the pollution problem (10), and so much has been written and said about it in the last year that we risk laboring the obvious even to mention it here. Among the many insidious and difficult aspects of the pollution problem, however, one not ordinarily stressed—lead poisoning—gives insight to its pervasiveness and urgency. The information on lead poisoning comes from several sources but is conveniently summarized by Lovering (11). It is now seemingly well established that lead poisoning was the reason for declining fertility and early death among noble Roman society (with effects quite different from those reported in American frontier society). The tolerable levels of lead in the blood, above which chronic lead poisoning occurs, are variously estimated as between 0.5 and 0.8 parts per million in the average person. Chronic lead poisoning causes lessened fertility, sterility, miscarriage, stillbirth, weakness, apathy, and early death. The amounts in the blood of persons in the United States range from 0.05 parts per million for rural non-smokers to 0.4 parts per million for urban heavy smokers. Analyses of grass adjacent to freeways show particularly high concentrations of lead (it is also reported that there is a high incidence of pulmonary diseases among children living adjacent to freeways in some areas). Quite apart from other factors that complicate pollution control, here would seem to be one that could easily be eliminated, and with concomitant conservation of a wasting resource—by finding economic means of producing anti-knock gasoline without the use of lead, or by using lower compression engines.

Certainly there are other aspects of environmental pollution involving hazards to health or quality of life that might be similarly eliminated while we are struggling to come to grips with the total problem. Air pollution in many cities is complicated by the industrial and residential use of fuels which emit waste gases containing sulfur dioxide. Sulfur dioxide reacts with atmospheric moisture to produce corrosive sulfuric acid. Of course, if the breeder reactor can be perfected, conversion to electric

power and heating generated from nuclear energy will eventually eliminate this problem and permit the conservation of organic fuels for other purposes. Meanwhile a promising goal is the invention of economically feasible methods for removing valuable sulfur from the gases in which it is harmful and applying it to useful purposes—as in the production of superphosphate fertilizers. It has been estimated, for instance, that the amount of sulfur emitted annually to the atmosphere in industrial gases throughout the world exceeds the total annual U.S. demand for sulfur (Ref. 12, pp. 347–348), so the potential resource is not inconsiderable.

Similarly, controls on the nature and use of detergents and biocides, although helpful, do not as yet go far enough. Phosphate, for instance, an essential plant nutrient in suitable concentrations, is harmful in detergents because it gets into the hydrosphere in excessive quantities, producing undesirably high and thus contaminating bursts of fertility among aquatic microorganisms and “trash” fish. Incentives should be created for the development of a detergent that will satisfy the home manager while conserving phosphate and keeping it to acceptable levels in surface and ground waters. Pollution abatement in general is one area of environmental engineering where incentives and penalties that focus attention on both potential gains and harmful effects are likely to be an effective means of stimulating constructive action in the private sector.

### *Analysis of Alternatives*

Much also has been said and written recently about cost-benefit analysis; and its limitations, hazards, and downright inapplicability to most research (particularly on a project basis) need no elaboration. In semi-quantitative terms, however, both the inherent benefits of reaching the target and the cost of achieving it by various methods can be usefully analyzed as a basis for developing research strategy. The price of oil in the United States, for example, is more than \$1 a barrel higher than it is in world markets, and some have estimated that, without import controls, low-cost domestic production and foreign imports might come to an equilibrium price more than \$1 a barrel lower than it is now, with a consequent saving of about \$4 billion a year to the American public. This, however, would be at a substantial loss to our current ability to supply our needs in times of emergency.

Nevertheless, there are other ways of protecting the national security with respect to petroleum supplies, besides maintaining domestic production and these too can be analyzed. Assuming they are not satisfactory, however, the estimated \$4 billion a year represents a target benefit for a solution that would, even if temporarily, yield adequate amounts of liquid hydrocarbons from domestic sources at or below world prices. Now, are there potential domestic sources large enough to meet the demand for

a period long enough to warrant their development, and are there foreseeable means to bring their production costs into the competitive range? Consider the following possibilities. Certainly there are some undiscovered resources that could be found and produced more economically if exploration tools and methods could be made cheaper and more effective. In addition, if secondary recovery processes could be much improved, a larger fraction of the oil in the ground could be recovered economically, and production costs could be lowered and supply increased for an appreciable time. Prorating and other conservation methods now add substantially to consumer costs (while achieving other purposes), and changes in regulatory procedures might also yield substantial savings to present consumers. For the long haul, moreover, there are vast if also finite resources of oil shale and coal, from which liquid fuels and other useful co-products (e.g., aluminum from dawsonite in oil shale) might be recovered profitably if more efficient processes were available.

The cost of these various solutions can be appraised only roughly, and more research is required on nearly all of them to evaluate precisely their potential. Even more difficult to appraise are other costs or gains that must be taken into consideration—effects on foreign trade and balance of payments, effects on the developing countries and our relations with them, and the social costs that might accompany the decline of a protected domestic industry. But complex and difficult as the problem of judging target benefits and achievement costs may be, it is subject to meaningful analysis.

The problem of petroleum depletion is paralleled in range and nature for many other similarly finite and thus also exhaustible mineral resources, and it highlights the need for coordinated analysis of alternatives. Factors to be considered for all such exhaustible resources are the magnitude of their reserves at different levels of concentration, extractability, economics, increasing rates of consumption, and possible substitute materials, as well as the whole range of societal and political factors involved, including the environmental degradation and pollution that so frequently accompanies their extraction and use.

There is both challenge and opportunity here to improve the process of strategic analysis and decision-making in the Federal Government. We can illustrate the tendency of the decision-making process to break down or simply not come into play where the environmental sciences are concerned by reference to a seemingly trivial but actually very critical problem—that of the starling. Now a national problem of considerable economic importance, the strategy should have been to prevent the extension of the starling instead of merely trying to drive it from the cities. Several years ago a distinguished ornithologist wrote a letter drawing this to the attention of a Federal official having the power to initiate action. This ornithologist pointed out that, owing to well-known nesting and



breeding habits of the starling, a suitable effort in seeking out and destroying outpost colonies could protect the western two-thirds of the country from its depredations. Nothing was done. Meanwhile the starling has reached the Pacific coast, has become a major threat to California vineyards, and has increased its annual toll of crop-destruction by many times what it would have cost to maintain an effective control program when the possibility of such a program was first pointed out. Here is a case where the consequences and eventual cost to the national economy of doing nothing should have been weighted carefully against the sizable but relatively trivial cost of preventive action.

In developing broad strategy for the entire Federal program, of course, the effort devoted to different aspects of the environmental sciences and engineering in support of various national goals must be weighed in the balance of urgency versus feasibility and cost of contributing operations. The intangibles and variables at this level are far greater than those involved in analyzing alternatives for meeting subordinate goals, and subjective judgment becomes a dominant factor in the process. Sound analysis, however, of the problems involved in and opportunities for subordinate goals can be the foundation for final judgments on the larger issues, and may thus be the most critical step in the process. Mechanisms should be provided, perhaps in the form of standing review boards for different aspects of the environmental sciences and resources, whereby such analysis can take place on a continuing basis as a guide to policy and action.

Finally, in relating the problem of relative urgency among Federal programs to the environment and its resources, two generalizations deserve emphasis. One is that the growing demands we are making on the environment and its resources, and the widening and deepening harmful effects of our use and exploitation of them, are rapidly increasing the urgency of finding acceptable solutions to problems of resource sufficiency and environmental integrity. The other is that, whereas the strategy for solving environmental problems may utilize a variety of methods, including regulatory measures as well as the application of science and technology, all require better knowledge and understanding than we now possess of our environment and its resources, of the interrelationships of natural phenomena and processes, and of their regional and historical development.

### ***Choosing Programs for Federal Support***

In a representative democracy, in which the Federal Government does for the people what they cannot do for themselves, many public ends are achieved by providing stimuli to the private sector, or in other ways creating the climate in which the private sector will accomplish public purposes of its own volition. This applies to research and development

in the environmental area as well as to other components of strategy. The first question, then, in implementing research strategy is: what needs to be directly undertaken or supported by the Federal Government, and what parts might be undertaken by the private sector at its own expense?

Analysis of this question with respect to the environmental sciences involves consideration of the types of research and development that non-government organizations traditionally do on their own, the means of encouraging them to devote more of their attention to environmental and resource problems, and, conversely, the types of research and development that the Federal Government ordinarily has to do or support itself.

Industry, of course, especially small industry, most often takes initiative on work that promises to return or maintain a profit in the short term. Where industry is concentrated in large companies or complexes, however, it may become more aware of its own continuity and future needs for raw materials, and consequently more concerned about reserves, improved recovery, and even conservation. Some oil companies carry out much long term and far-ranging research, some large lumber companies have adopted sustained-yield plans, and even banks and public utilities are becoming conscious of soil and water conservation as factors in the general prosperity that keeps them going. The attention of industry can be drawn to specific problems or fields in various ways—for instance by appeal and persuasion, both of which are more effective instruments of policy than is often recognized. Alternatively there is the feasibility demonstration, suggesting that a profit can be realized through research and development in a given field, for instance the various pilot plants the Bureau of Mines has operated. Finally there are regulatory means that put the private sector at advantage when it does one thing (e.g., income tax provisions that encourage research), or at a disadvantage when it does something else (e.g., pollution-abatement regulations that may stimulate research to avoid the penalties involved).

Examples of environmental problems that justify the attention of industry in its own interests include, for instance, the recovering of pollutants profitably and the development of new sources of commodities in short supply (or substitutes for them). It is worthwhile then continually to explore possible incentives that Government might provide to encourage industry to undertake more research of its own in the environmental fields.

Universities, acting on their own initiative, tend to focus on problems stimulated by the science itself, but the interest of university scientists in national or regional problems can often be aroused by publicizing the need for research to meet specific goals—particularly if the appeal is backed up by financial support for relevant research.

The Federal Government should itself, therefore, conduct or sponsor primarily research and development that is too large, complex, or costly for the non-Federal sector to undertake, that needs to be accomplished with much greater rapidity than could be achieved otherwise, or that needs to be done with a system or a standard that the private sector is not likely to provide. The essential regional surveys and time-referenced research are prime examples within the environmental sciences of work that fits nearly all of these criteria. Moreover, the difficulty and complexity of topical problems in the environmental sciences ordinarily put them beyond the fringe of problems for which there is sufficient profit motive to attract industry and may, therefore, require that research and development of such orientation also be supported by the Federal Government in its own or in university laboratories.

Inasmuch as all national goals involve some environmental constraints, and since the integrity and proper use of the environment and its resources are the traditional and proper concern of governments, there should be clear Federal recognition of responsibility in this area. There should be mechanisms for maintaining continuous review of all significant activities relating to the environment and its resources throughout the Nation, and for assuring that necessary studies and action are undertaken, if not by the private sector, then by or with the support of the Federal Government itself. The growth of Federal support for oceanography illustrates one possible pattern, with a diversity of essentially national laboratories under private administration coupled with other activities undertaken by strictly Federal laboratories. This structure needs revision leading to better integration and focus, but not the elimination of fruitful competitive overlap. Similar developments, profiting from experience, should be extended to other aspects of environmental science and engineering.

In so doing it is important to keep in mind that scientific brains come in all hierarchies of capability, creativity, and susceptibility to differing stimuli. Different people respond to different mixes of motivation. Variety in organization, therefore, as between and within Government, university, and private circles is a positive good in that it offers widened opportunity for original thought and interaction. Applicable knowledge, moreover, is most likely to be recognized within the group or organization that is motivated to find it. Hence a variety of organizations with a variety of missions is one means of casting a wider net for applicable science, provided that organizations are not insulated from interaction with each other or with the universities and the scientific community in general.

#### *Balance within the Research and Development Spectrum*

How funds for a given program should be allocated across the research and development spectrum—how much for research that is only loosely

related to the mission, and how much for engineering, development, and other immediately practical objectives—are problems without clearcut or permanent answers. For the environmental sciences there is both need and opportunity to develop applicable science and apply it more widely. Many of the currently pressing environmental problems for which the Federal Government bears heavy responsibility are ones that require emphasis on the applications of science and on advances in engineering and technology—they include all aspects of environmental pollution, water supply, and increased production and better use of all kinds of resources.

To overemphasize such immediately practical activities, however, at the expense of more wide-ranging mission-related and internally generated research, would involve grave risks, for in this latter component of the research-development spectrum lies the capability to pursue long-range objectives and the important flexibility to meet the unforeseen. It is in the long-term aspects of its environmental objectives that Government science should maintain the greatest internal strength, even though it may find it expedient in some areas to expand its activity for more immediately practical ends. The greatest good it can promote is to maintain the largest possible flexibility in options for future choice that is consistent with effective interim uses.

Thus, it is essential to the healthy development of the mission-oriented agencies that they support a broad spectrum of research that goes beyond that obviously related to their immediate mission, among other reasons so that they can evolve toward newer and more crucial missions as the old ones become redundant or obsolete.

Just as a wide array of medical specialists is required by a great urban complex, so is a wide array of scientific talent important to the Federal Government, both to assure the availability of particular talents when they are needed and to explore widely into areas that involve risks not acceptable to individual private enterprise. It is thus eminently logical, for instance, for the Public Health Service to have on its staff specialists in the minute marine organisms known as dinoflagellates, or to support dinoflagellate research in the universities, simply so that someone may be available and have the requisite knowledge to deal with shellfish pollution and red tides caused by such organisms as problems arise. Wide-ranging research on viruses and rickettsiae is, and should be, carried out in and supported by the Department of Agriculture, because some of these minute sub-organisms are involved in destructive plant diseases. A staff of entomologists also has obvious relevance to environmental problems. And bird migrations should be studied, among other reasons because they may be the vectors for disease carrying viruses.

It is also appropriate, as another example, for the Geological Survey to have on its staff, or to support elsewhere, specialists who do unprogrammed research on various fossil organisms as well as other specialists

in nuclear geology, if for no other reason than that either or both may be required in determining the ages of rocks of practical interest. Such information is often of paramount importance to mineral exploration or to guide construction. For instance, the simple recognition a decade or so ago of a Devonian fossil in a drill core from a Geological Survey test hole that penetrated covering lava rocks was largely responsible for the discovery of large concealed lead-zinc-silver ore-bodies in the East Tintic district of Utah. These ore bodies were subsequently estimated to have a value well in excess of \$100 million. In another application of "overhead" paleontology, the identification of fragmentary fossils as distinctive Cretaceous forms led to recognition of a large thrust fault and the rerouting to a safer location of the 22-mile-long Roberts transmountain water-diversion tunnel west of Denver.

It has been shown repeatedly by successful practical research that the in-house interplay between internally and externally inspired research is essential to success. The ratio between operations called basic and those called applied in the best industrial laboratories has been identified as commonly about 1 in 10 but occasionally as much as 1 in 3. However, because industrial laboratories draw on the whole external scientific community for research background, a higher ratio should prevail in Government supported laboratories, which have a broader and longer time-range function in the application of science to national goals.

### **Management of Federal Research in the Environmental Sciences**

Management of research and development in the environmental sciences, is a problem that particularly concerns the Federal Government, not only because there are important objectives to be met, but because of the Government's special and large obligations in this realm. Although problems such as where and in what institutions the work should be done, how it should be organized and directed, and how its performance can be appraised are ones that apply to all research and development, it may be of interest to consider how they appear from the viewpoint of environmental science. If our remarks seem to have a broader bearing, that is not surprising, for the important elements of research management do not differ greatly from one field to another.

#### ***Matching the Operation with the Institution***

There is, of course, good reason to support environmental research and development in many kinds of institutions, but what are the criteria for judging what activities are best assigned to each? Briefly stated, the universities seem to do best with free-wheeling innovative research not tied to specific missions, industrial laboratories are oriented to production and operations and hence best suited for engineering and development activities (although they may also do fundamental research of deep signifi-

cance), and Government establishments are best suited to undertake long-continuing mission-oriented research and surveys.

Much short-term mission-directed research is, of course, appropriately carried out in universities on Federal project grants. In parts of some universities, however, contract research has become a very large activity. There is cause for concern that, in such instances, it may be displacing the science-directed, individually generated, innovative investigations that we look to the universities to provide. It is important for the Government, the universities, and science that this not happen, but that the universities maintain instead a good mix of mainly internally generated, science-oriented research as an important adjunct to their educational function and the Nation's science underpinning.

The necessary large-scale and continuing Federal environmental research is currently done in both in-house and in contract laboratories. The contract laboratories (which do other things as well) have essentially the same strengths and capabilities as the in-house laboratories, and some of them need to be maintained for the purpose for which they are apparently intended—flexibility in emergency to act promptly and employ key scientists without the fetters of Government's own civil service and procurement regulations. But if this same purpose justifies their expansion in peacetime (and it is hard to think of any other justification), then Government needs to examine closely its own inherent competence to run any research institutions at all, or any other enterprise, for that matter, that involves the use of highly trained scientists and engineers in important missions. Admittedly of course, it is difficult for Government to operate with the flexibility of the private sector, and there are continuing weaknesses in the Government's administrative procedures that make it simpler to do certain things outside. The contract laboratory, however, is inherently more expensive to operate and more difficult to control. The challenge to Government, therefore, is to reduce and eventually to overcome its procedure weaknesses so that it can carry out its own research in-house with satisfactory operational flexibility.

Whether done in national laboratories or Federal agencies, or on contract to universities or to industry, however, we can endorse the concept of competing operations defended in other essays of this series by Weinbery and Teller. The problems of environmental science are too complicated and too important to entrust to a single agency free from the counter checks, tests, and stimuli to seek alternative solutions that arise from the existence of competitive operations.

### ***Creating Effective Operations***

Good management in science has much the same purpose as government in a democratic system. Its purpose is not so much to govern as

to produce a climate favorable for individual initiative and the creativity it yields. In the environmental sciences it is imperative that the agency itself have a record of long-continuing good management if it is to gain the full value of having the same creative scientists work at long-range programs over long periods of time.

Given adequate physical facilities, the effectiveness of a research institution, in or out of Government, thus is likely to be primarily a function of the quality of its leadership and scientific personnel, and the intellectual environment and physical surroundings in which they work. The director of a laboratory or research agency is in large part responsible for the intellectual atmosphere, and giving him also the weight of responsibility for direction and relevance of the program requires that he be a strong, independent, flexible-minded scientist. Secretary John W. Gardner recently listed as rules for maintaining and renewing the health and strength of an organization: effective recruitment, a hospitable environment for the individual, an atmosphere receptive to self-criticism, fluidity of internal structure, adequate internal communication, flexibility of procedure, ability to combat internal vested interests, a deep concern about the organization's future, and high motivation on the part of the staff—a belief that what they do really matters. The agency or laboratory director is the executor of these principles and his selection is perhaps the most important single step in achieving excellence in a research institution. To assure that he is capable of providing the leadership required it is desirable that such appointments in the Federal system be made on nonpolitical grounds from slates of qualified candidates prepared by committees of professional groups such as the National Academy of Sciences, National Academy of Engineering, other national professional societies, or by non-governmental agency advisory committees.

Although good internal scientific leadership is critical in determining the quality of a research organization, however, many Government procedures and regulations beyond the director's control hinder the effective development and operation of a Federal research agency. The recruitment of outstanding young scientists, for example, is made excessively difficult by the timing of the budget process. Because the budget ordinarily is not approved until late in or after the close of the school year, the best of the new graduates are likely already to be committed elsewhere by the time there is assurance of funds to hire them. The pattern of appropriations for many agencies, moreover, seems to be the feast-or-famine one, in which several years of no increases and little or no opportunity to take on new blood are followed by a substantial program increase that can be staffed only by dipping to or below the level of mediocrity. And as the mediocre acquire retention rights, the danger of residual impoverishment grows, for the security of Federal employment has a stronger appeal for these than it does for their more sought-after colleagues.

Federal salaries, better than they once were, are still not competitive with contract laboratories; and the Public Law 313 positions, created to enable Government to draw to or hold a better share of top scientists in its own laboratories, simply are not available to many agencies in sufficient number to be significant. Many of the regulations governing staffing, travel, and procurement, moreover, make it difficult to operate a research institution efficiently. Although, in theory, recent regulations make it possible to provide self-renewal through training programs and recognition through incentive awards, in practice they have not been well adapted to the needs of scientists. Rules governing, and operational practice in providing, self-renewal, advancement, and recognition generally would profit from more frequent review and comparison with external practice.

In spite of these and other built-in obstacles to excellence, some Federal research institutions are truly outstanding. We suspect that the secret of their success lies in continuity of good leadership. Considering the system in which they operate, it would probably be more meaningful to ascertain why those few are superior, and why distinguished scholars remain in their service, than to ask why others fall short.

In sum, the environment for research in Federal laboratories could be improved to draw and hold a more suitable selection of the best scientists and engineers by giving them top scientific leadership, minimal and enlightened management, strong incentives, adequate technical and sub-professional support, opportunity for self-renewal, recognition, and freedom from petty harassment. Scientists are motivated by many incentives—opportunity to do original work, freedom to choose it, opportunity to serve, recognition, money, and congenial circumstances (including administration, services, facilities, and space). That most of these can be provided just as well within the Government as outside it is demonstrated by some of the truly outstanding Federal research organizations.

### **Sources and Conservation of Brainpower**

In view of the vital role that science and engineering play in the public purpose, not only in the environmental sciences, but elsewhere, the Federal Government must seek to meet needs for additional brainpower in advancing science and engineering. What goals are feasible depend not only on the state of knowledge and instrumentation, but on who is available. If we wish to widen our choice of practicable goals, we must broaden the spectrum of brainpower capabilities. In its early land grants, tax benefits, and other aids, and more recently in its institutional grants, contracts, and scholarship programs, the Federal Government has played a positive role in developing scientific and engineering brainpower, but the needs have increased faster than the means to meet them, and the means



should be further increased. We do not suggest that creative minds can be developed on order, but that, with training, experience, and opportunity many minds can learn to function at higher levels and some unrecognized creative minds will emerge. In any case, education is never wasted as long as it goes on at a sufficient variety of levels and intensities to allow its products to find the level and intensity at which they perform best.

There are, of course, both short-term and long-term aspects to the problem of scientific brainpower, and they must be met in different ways. At any given time, the total brainpower pool is essentially fixed in size, but substantial shifts within it are possible in shorter periods to match shifts within the total program—for example, the recent development of space research. Many scientists are, quite properly and wisely, so dedicated to their on-going investigations that they are not readily interested in diversifying or changing their research directions. Others, however, can be attracted by the opportunity to explore new horizons, especially in an imaginatively conceived program that promises interesting results, recognition, and financial support for equipment, assistance, and services. Movement of brainpower from one area of science and technology to another is an old and successful way of meeting new demands, and for the environmental sciences in particular, movement to them of people trained in the primary sciences can be enriching and rewarding for all concerned.

Another short-term solution that should be a long-term operation is to increase the efficiency of use of existing brainpower. Computers and other technologic advances that reduce the need for people have had many beneficial effects here, and there is much room for improvement in the utilization of such aids in the environmental sciences. Widely neglected however, is the need to increase efficiency of scientific brainpower by providing scientists with more sub-professional and clerical assistance. In the environmental sciences in particular there is need for a great deal of data gathering, manipulative, and compilational work which, if done with care and under suitable scientific direction, does not require the highest level of creative mind. In many laboratories, both in and out of Government, a large increase in efficiency could be obtained by providing funds and positions for such assistance without adding a single scientist to the staff.

For the long-range, the problem of increasing brainpower is partly one of improving the quality of science education, and partly one of identifying and utilizing more fully the latent scientific brainpower in the population as a whole. The extent to which the percentage of scientists and engineers in the total population can be increased is, of course, difficult to judge. Certainly an upper limit is imposed not only by individual qualifications of mind and character, but also by the preference that many people with suitable qualifications have for other occupations, and by so-

ciety's need for them in other fields. In womanpower and among currently underprivileged groups, however, there is a substantial reservoir of potential scientific brainpower not now being effectively utilized.

Improvement in educational capacity and quality is a goal to which the Federal Government can and should contribute substantially through several existing mechanisms. Here, however, we wish to bring attention to an opportunity that is of particular interest from an environmental viewpoint. It is often observed that rich industrial and cultural centers and great universities tend to be associated, although the ratio is not 1 to 1, nor is it clear, when it exists, which is cause and which effect. It would be interesting and probably fruitful to try some experiments to see whether such a complex could be consciously created.

Estimates of population growth in the United States, taking various factors into consideration, converge to suggest a near-doubling of a total of about 360 million by the end of the century. Trends in urbanization and the demand for higher education imply that more than twice as many people as now will then live in cities, while the enrollment in colleges and universities may have increased nearly three times. Are all these people to crowd into existing cities and schools, leaving half the country's land with fewer than 10 percent of the people, or are there constructive alternatives? Why could not vital new industrial and cultural complexes be made to grow up around needed new universities and Government laboratory centers at places such as Ely, Minnesota, or Santa Fe, New Mexico, for instance, and not just at the fringes of existing urban sprawls? If such new centers could be created, they would present at once a relief to pressure for continuing growth in existing urban areas, a major contribution to internal stabilization of the Nation, and an opportunity for experimentation with new and better designed urban systems. Concurrently they would enhance the Nation's higher educational capacity and provide other opportunities for experimentation with new educational patterns. In preparing to serve our exploding urban and university populations we will need all the capacity and new ideas we can muster until such time as man can come more nearly into balance with his surroundings, as eventually he must.

A further significance of the suggested new universities with regard to the environmental sciences, is that such institutions might generally be situated in regions where the local environment offers special problems or opportunities. The sea-grant colleges, already proposed, are examples of structures that might not only advance oceanography as a science, but contribute to the special needs of indigenous enterprise. Similarly, new universities in now-rural regions should include or be associated with special institutes for the environmental sciences that would interact with the regional environment and its resources so as to aid economic development in lagging sectors of the Nation. Of course we do not mean

to imply that the creation of such institutions is an alternative to strengthening existing ones. Both must be done. Nevertheless, in view of the large population increases and educational demands that will yet come before leveling-off is feasible, and of the increasingly urban nature of the population in general, more first-class universities and more urban centers, more widely distributed would enhance the quality of life throughout the Nation. It would be eminently logical to try to nucleate the needed new urban centers around new "urban-grant" universities.

In view of the need for better and fuller application of the environmental sciences, moreover, more thought needs to be devoted to educational requirements in their engineering phases. The engineering phase of many environmental sciences is not now recognized either in name or training. Perhaps part of the reason for this is that the need for a geological engineer, for example, is not for one who can double for a civil engineer in laying out the foundation specifications for a bridge or a dam, but for a first-class geologist able to analyze the complexity of the foundation setting and to translate it into terms relevant to the construction problem. Whether or not the ability to apply environmental science is recognized in a separate name may not be important, but it is important that the capability be further developed. With suitable support, fellowships, and renovation this may be a function that institutions like some of our now-moribund schools of mines across the Nation could serve. However, scholarship and fellowship support for persons undertaking suitable courses of study or postdoctoral internships in any institution would be a constructive move toward the advancement of environmental engineering.

What steps, then, can be taken to assure that new creative talent is utilized effectively, that applicable new knowledge is recognized without delay, and that technological obsolescence is minimized? In detail there are many. Most, however, follow automatically under policies that give positive encouragement to increased interchange of persons (including students) between Federal and State laboratories, private institutions, and the universities as well as to more direct linkages between Federal and State institutions and the universities in terms of training grants, fellowships, and collaboration in research. It is inevitable that occasional abuses will arise under such a free-exchange system. To reject the system or make it unduly cumbersome on that account, however, would be comparable, as some wag has put it, to throwing out the baby with the bathwater.

### Summary and Recommendations

The environmental sciences develop knowledge about all aspects of the environment, including natural resources, and contribute to man's ability to understand and interact effectively and sensibly with his world. The environment and its component resources represent an interlocking,

dynamic system, in which man himself, in using it and seeking to gain control over it, has become its dominant element, and is profoundly modifying it in ways that are often not anticipated or understood. With the growing demands we are making on this system, and with the widening and deepening effects of our actions, it is important that we greatly improve our understanding of our environment and its resources, and that we more widely and effectively utilize such understanding to arrive at a better balance with nature and to insure a flexibility of choices in the future.

Most of the problems we face as a nation, and most of the goals to which we aspire, are closely linked in one way or another with our use of the environment. Far better knowledge than is now available is needed if we are to identify and solve problems relating to rapidly increasing demands for minerals, fuels, water, food, and fiber; to pollution, recreation, preservation of scenic values, and public works; to the effects of natural hazards; to control of many diseases; and to a host of other aspects of the environment. The primary sciences and their associated technologies, of course, will provide the levers intended to bring many of these problems under control, but they can do so effectively and safely only in context with a deep understanding of natural systems and the consequences of disturbing them.

Most environmental problems are complex, not only in the dynamic interaction of many processes, but also in the human activities associated with them and affecting their resolution. It is essential that research strategy bearing on such problems be formulated in full coordination with the development of regulatory and other policies that may affect the outcome, with attention both to the short- and the long-term. In this meld of policy, it will often be found that regulatory procedures represent a short-term palliative, whereas research, engineering, and the applications of social science offer the greatest potential for long-term cure, including the satisfaction of seemingly conflicting requirements.

In seeking balance over the research-development spectrum, there is need in the environmental sciences for more mission-oriented research and engineering, but it should not come at the expense of wide-ranging research that probes deeply into fundamental relationships. Indeed it is this self-stimulated research that will eventually yield the best long-term solutions, and that provides the flexibility to meet unforeseen problems and preserve choices for posterity. Creative diversity, dynamic balance, and plurality of options for the future are the objectives to be stressed.

The environment impinges so extensively and inescapably on the lives of all citizens, and transcends so generally all private spheres of interest, that much of the research required in the environmental sciences is necessarily carried out by Government-supported institutions. The needed

regional and time-referenced surveys are essential Government functions, and many other environmental problems are of such long-range character, or so closely related to survey work, that they are not easily carried to conclusion under other auspices. Moreover, they are either too complex or costly, or offer too little profit-incentive, to be undertaken by industry. Research that may be mission-related but is not mission-oriented should make up a substantial part of the in-house research in such Federal, State, and regional laboratories, both for balance and for flexibility to meet unforeseen needs. Much also should be supported by various levels of Government in the universities. Good commercial laboratories that have specialized in problems of engineering and technology in the environmental field should be utilized where possible, and increased use of them by the Federal Government might encourage the development of greater research capability in the environmental industries.

The quality of existing Government operations in the environmental sciences ranges from outstanding to substandard. Principles of good science- and engineering-management are not uniformly applied, and many practices and some regulations weaken the agencies' effectiveness. The administration of environmental science and engineering in Federal institutions needs continuing attention. A constructive way to provide such attention would be to identify the factors responsible for the achievements of laboratories or agencies recognized as outstanding and attempt to introduce such factors more widely where applicable. Enlightened management, opportunity to interact, and a large element of freedom of choice are surely among the factors that make for a consistently high level of achievement. Careful selection of agency and laboratory heads from slates of qualified candidates prepared by professional groups is one of the best ways to assure that these elements are present.

In view of the role that science and technology play in all phases of our society, the Federal Government has clear responsibility to contribute heavily to the discovery, development, and effective use of scientific and engineering brainpower. More Federal aid, therefore, is needed for institutional and program development. In addition to strengthening existing institutions, however, a populace expanding towards twice its present number and three times the present number of college and university students by the end of the century needs new universities in geographic settings in which they can, on the one hand, contribute to the nucleation of new urban centers, and on the other, aid in meeting the expanded needs for education and the solution of regional problems.

Throughout all operations steps should be taken to assure the effective use of creative talent, the prompt recognition of applicable knowledge, and the reduction of technological obsolescence both in individuals and in institutions. The interchange of persons and ideas between all

kinds of institutions at all levels is one of the most effective means to assure such ends. The needed new operations, more fruitful linkage between old ones, and creation of more appropriate opportunities and incentives might be facilitated by establishing external overview boards for the main aspects of the environmental sciences and then paying attention to their recommendations.

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# APPLIED SCIENCE AND MANUFACTURING TECHNOLOGY

by DONALD N. FREY and J. E. GOLDMAN

## Introduction

In its broadest scope manufacturing is concerned with the conversion of matter and energy into useful products for markets. It encompasses those phases of industrial operations that deal in volume with the preparation of raw materials conversion into useful shapes, processing into products and devices, and assembly into complex systems. Any one or any combination of these operations can be the basis for a manufacturing industry. Any discussion of research and innovation for processing and manufacturing thus deals with the core of much of our industrial economy.

Applied research in manufacturing technology is generally directed toward accomplishment of the following objectives:

- a. manufacturing procedures to attain new specifications and performance standards
- b. improvement in quality, reliability, and uniformity of final output
- c. decreased cost of manufacturing operations

The means of production of material goods represent significant interactions of research and technology. However, much of our widely publicized technological innovation generally relates to new materials, new devices, and new complex engineering systems. The briefest reflection, however, indicates that obviously no such material or device would have serious industrial or commercially economic significance if it could not be produced on a scale and at a cost compatible with consumer markets. The laboratory discovery of a material with exciting properties would remain only an intriguing curiosity if processing and manufacturing methods could not be developed to produce such a material at a cost to satisfy market requirements. A typical case is provided by metal and ceramic whiskers, which exhibit superstrength properties but present enormous technical and cost difficulties in incorporating them into useful products in sufficient volume. Titanium, beryllium, molybdenum, niobium, and other refractory metals would still be considered exotic materials if commercial processes had not been developed to produce them in sufficiently purified massive form and to fabricate them into useful

shapes. Indeed, much of our commercial vacuum technology now commonplace in the metals industry stems from the requirements for rigorous control of impurities of these "reactive" metal systems. It is not unexpected that much of our technological sophistication is characterized solely in terms of the new products we use. A host of new materials and devices excites consumer appetites, but market success is predicated primarily on the development of processes permitting economic conversion of these materials and devices into useful products. The wonders of the transistor are now widely known, but without innovation of zone melting and diffusion processes into manufacturing methods, the transistor would be little but a scientific curiosity.

Research on manufacturing and processing technology is applied research, with extensions into engineering and development. The goals of such research are usually clearly defined and while the approach and problem, *per se*, may have intellectual structure, the results are judged primarily in terms of utility. Much processing and manufacturing technology may have only historical and/or tenuous connection with science. But it is becoming evident that more recent significant innovations in manufacturing methods are science-based. This is particularly true of individual manufacturing processes concerned with control of chemical composition and/or control of physical structure and shape. Over the years there has been a continual evolution from control on the macroscopic level to control on the microscopic level. There are excellent examples of this in the progression from the electron tube to the transistor; from conventional paints to molecularly engineered polymeric coatings cured by electron radiation. Continuing evolution of manufacturing requirements will produce increasing demands for control at the atomic level. This will result in more sophisticated processes, including increased instrumentation for control and inspection. All this implies an increasing dependence of manufacturing research on contemporary or recent science.

### Product-Process Relationships

Much of the social and economic impact of science and technology has been a direct result of our ability to create new and useful products. Indeed the major thrust of science to technology has been in the direction of the invention and design of new materials and new products. This has been a predominant characteristic of much of the federally supported research and development activities of the past two decades. With a few singular exceptions, attention has been focused on properties, behavior of materials, and characteristics of new devices and new products rather than methods of production or processing, or even less, improvements in established processes and products. This may be observed in the character of Federal support to metallurgical science and engineering. An



analysis of one of the chief publications of the professional metallurgical community in the United States, *Transactions of the American Institute of Mining, Metallurgical, and Petroleum Engineers*, for the year 1964 indicated that ten times as many papers were published in physical metallurgy as in process and extractive metallurgy. This fact dramatically attests to the popularity of physical metallurgy versus process metallurgy, stimulated to a large extent by attitudes of Government research-supporting agencies. Only 23 percent of process metallurgy research was Government-sponsored, as compared to 60 percent in physical metallurgy. The recognition of the existing imbalance, and its impact upon the academic research and educational system, particularly with respect to its implications of sources of future manpower for the metals producing and processing industry, led to the issuance of the "Derge" Report by the American Institute of Mining, Metallurgical, and Petroleum Engineers, calling for a redress of the situation. These circumstances resulted in congressional legislation authorizing the Bureau of Mines to administer sponsored research programs in extractive and process metallurgy. The situation with respect to metallurgical engineering is quite typical of the focus of most fields of engineering on product design, though it must be noted that chemical engineering, in which an appreciable emphasis is on process, is perhaps the major exception, much to the benefit of the chemical industry. The petroleum industry, for example, historically has been concerned with the conversion of natural petroleum resources into refined products, and the bulk of its research has thus been focused on process-control features for practical and economic methods of conversion.

In contrast to the emphasis of Government support on materials and products, a more substantial focus on research related to processing and manufacturing can be found in research supported by industry, particularly in the consumer product industries. This is undoubtedly a reflection of utilization demonstration as the ultimate test of research innovations and of the economic gains associated with process improvements in high-volume production. The intimate relationship of industrial research with the marketplace forces an unquestioned necessity upon the translation of materials and product-research innovations into producibility and manufacturing feasibility. This, we believe, may be responsible for the intrinsic differences in "coupling" (research-technology interactions) effectiveness underlying research activities in industrial enterprises as compared with those in Government laboratories or Government administrative staffs. It is difficult to "couple" meaningfully in the abstract, and the ultimate industrial test of utilization, which often rests upon processing and manufacturing feasibility, provides the environment for the subtle interplay between research and engineering and the sometimes elegant "feedback" interaction between science and technology. But industry is not immune from reproach in this same area. Industrial research for

improvement of processing and manufacturing technology often is also a neglected stepchild.

It is one of the principal objectives of this paper to demonstrate, by illustrative examples, the economic opportunities and potentials available through the implementation of a science-based technology for innovations in manufacturing methods. Over the past decades the United States has led the world in the invention and development of new products and new technology-based systems. To a large degree we are just beginning to experience the impact of these new technological developments. Successful exploitation of their full possibilities may well rest upon our ability to reduce the design and product concepts to economic hardware and systems. This, in turn, will depend on our ability to devise and develop manufacturing and processing innovations. This will require some shift in the thrust of applied science during the next few decades from design and product to process and manufacture. A fundamental issue for the economic future in meeting foreign competition rests upon our leadership in innovating in the manufacturing function.

### **The Character of Applied Research in Manufacturing Technology**

Applied research is the bridge between science and technology; it is both science and technology and an interface between the two. At the science end of the bridge we have taken the boundary between pure and applied research in terms of mission-orientation. In this discussion therefore, mission-oriented research is applied research however basic or however scientific it may be. At the technological end of the bridge we have set the boundary between applied research and product engineering in terms of responsibility of applied research, which is to demonstrate feasibility of devices, systems, materials, and processes. We, therefore, consider that any engineering development necessary for the demonstration of feasibility is applied research, no matter how design-oriented it may be.

By demonstrating the feasibility of an innovation, we do not mean simply a laboratory demonstration of a concept or a principle. A demonstration of feasibility requires the presentation of solutions of problems in the design of devices and systems, of quantitative relationships in materials and processes, with the essential data for the design of special processing equipment. Modern manufacturing technology requires the close integration of product design and manufacturing methods for optimization of the manufacturing function. The essence of innovation in manufacturing methods therefore involves a trinity of materials, processes, and design.

The ten illustrative examples presented in the latter part of this paper illustrate the wide range of variation in the degree to which useful innovations have involved research in systems and devices along with re-

search in materials and processes. The evolution of the basic oxygen furnace and thin wall casting as processes in the steel and iron industry exemplifies process improvements that modified the economics of a classical material. Process research that made possible new and useful materials is exemplified by the development of Corfam and Nylon and titanium carbide cutting tools. New processes applicable to a wide variety of finished goods processing are represented by the electrocoating and radiation-curing coating processes. The zone purification of semiconductor materials illustrates a process development essential to an extensive and complex program of innovation—the transistor. The development of microelectronic integrated circuitry illustrates the situation in which innovation in systems, devices, materials, and processes are quite inseparable from one another. The utilization of existing science to establish a new process technology for fabrication of such conventional metals as copper and steel is well demonstrated by high-pressure metal forming. An industrial product development that took advantage of processing and materials innovation in other industries by the disc-armature-type motor.

Other examples can be given in which the need is apparent but the result has not yet been achieved. For example, if processed titanium metal was available at a cost near its unprocessed cost of \$1 per pound, the economic benefits would be such that, for all intents and purposes, titanium so produced would be an entirely new product. These examples help us to identify certain characteristics of the applied research process when directed toward manufacturing improvements as distinct from the perfection of new materials or devices.

1. *The scale factor.* One factor which often inhibits research on manufacturing methods is the relatively large equipment and scale-up costs involved beyond the direct laboratory stage. Production feasibility requires pilot plant size facilities which require investments an order of magnitude greater than laboratory scale experimentation. This is particularly true in the primary metals industry and related fields where tonnage scale experimentation is needed to demonstrate process verification. Experimentation on continuous casting, thermomechanical processing of metals, new steel-making processes, and the like require major resources of capital (e.g., see Example 6, Basic Oxygen Steel Process). Often the over-all economic environment, i.e., the current profit position of the organization, plays an important role in decisions concerning production-feasibility experimentation. It is at this point that the capacity and willingness of the industrial organization to innovate meet an early test. Another inhibiting factor in the translation from laboratory findings to plant operation is the reluctance of plant management to interrupt production schedules and tie up production equipment for research personnel. Unfortunately there are few research laboratories that have equipment and facilities to carry on research on manufacturing methods

on the proper scale, and it is a rare company indeed in which the research operation has the authority to interfere with production operations. It is encouraging to note, however, an increasing trend toward equipping of research laboratories with the necessary facilities, albeit expensive, to conduct full-scale experiments under carefully controlled conditions.

2. *The subtlety and interdisciplinary character of the applied-research process in manufacturing.* The titanium carbide cutting tool material—a major advance in metal cutting—was developed by a ceramics group in an automotive laboratory that was, in fact, seeking a new material for turbine blades. The same laboratory developed a new battery as an outgrowth of a program of the basic science germane to the processing variables in glass-making. The development of high-pressure metal forming drew upon high-pressure physics and the intricate metallurgical phenomena of ductility and fracture. Electrocoating required the infusion of polymer chemistry and classical electrostatics into a subtle engineering application. Device and material development often can be undertaken by a small group with a specific goal and disciplinary orientation. Process development, by contrast, invariably invokes a host of scientific and engineering disciplines.

3. *Importance of the individual.* Most developments can be traced to the tenacity and dedication of particular individuals (or organizations) who, having recognized the applicability of a new or old scientific principle to a process, were prepared to go well beyond the laboratory stage to prove feasibility, economic utility, and viability, which in the case of a process, means carrying right into the assembly line or machine or sometimes setting up a pilot plant right in the laboratory. Zone melting, electrocoating and titanium-carbide tools can all be traced to the tenacity of such "technical entrepreneurs."

4. *Economic constraints.* The economic barriers in process development are formidable. Product costs are already under great competitive pressures. Demonstration of feasibility of a process often must necessitate the interruption of an assembly line or the expenditure of an inordinate amount of funds on pilot runs. In consumer oriented industries, investment in processing improvements must often be assessed in the context of alternative investment opportunities in product expansion or improvement. Perhaps most prevalent of all constraints on investment in processing research is its magnitude relative to realizable economic gains if applicability of a new process is limited to a given company or even an industry. Many new processes, to justify their investment, must be utilizable beyond the product area of the company that hatches them.

5. *Progressive and open-minded management.* The risks attendant upon demonstration of feasibility of a new manufacturing process place the same demands on the faith and tenacity of management that are imposed on the technical man. It is management that must be prepared

to accept a sacrifice in this month's profit-and-loss statement in order to support high expenditures in time, money, and productivity in order to buy futures in efficiency.

6. *Systems approach.* Applied research in manufacturing processes leads not only to a demonstrable hardware concept but to an ultimate economic advantage. This advantage, taken by itself, may not be immediately apparent. A new method of joining metals may be more expensive than simple welding or bolting, but resulting savings in weight may derive other benefits in product performance or transportation costs. Quality advantages that are more expensive may reflect themselves in decreased warranty and policy costs several years later. A numerically controlled machine tool or new cutting material may prove more expensive than the tool or material it replaces but may reflect greater savings in down-time. To effect maximum recognition of the usefulness of applied research on manufacturing processes, management must take a systems approach to evaluate the utility of the process (e.g., see Example 8, Microelectronics).

Perhaps the greatest impact in this approach to processing research is yet to come from automation of processing through computer controlled system control. Process system control compatible with computer needs of a modern business enterprise now becomes possible and feasible with the emergence of third-generation computer hardware and command and control software. The possible impact of these developments in direct control of manufacturing processes and in the integration of separated manufacturing facilities and the indicated avenues of applied research in this area are detailed in our later discussion of automatic process control.

7. *Limited publication opportunity.* Unlike the basic researcher whose "stock in trade" is publication in professional scientific journals, the applied scientist and process development engineer have limited channels for such professional recognition. Commercial and proprietary implications of technologically oriented programs delay, and often suppress, publication in recognized journals, even of science based aspects of the work. This may be particularly true for small companies, in which state of the art know-how provides a major competitive advantage. Intellectual and professional recognition must be provided through other means.

### **Applied Research and Reliability**

A major, and too often unrecognized, role of applied research lies in its contribution to product reliability. Reliability may be considered simply as the capability of a specified performance within a given set of operating conditions and environments for a specified time period with the minimum of requirement for unscheduled repair and adjustment.

For a complex product, reliability of materials, components and the over-all equipment system necessarily is involved. A complicating factor in reliability consideration is that operating and environmental conditions are specified by the product user or customer according to his usage pattern, which realistically may not correspond to the service requirements anticipated by the manufacturer. Thus one of the objectives of reliability engineering is to determine what are real-life operating conditions so as to establish methods and test procedures for measuring and assessing reliability parameters.

A product that is produced in large volume can be tested over its required life only in very small relative numbers. Consequently every possible use must be made of the information relating to durability factors in actual widespread product service experience. From this information specifications and test procedures can be developed that can be directly applied to similar existing products, and use conditions can be inferred from which the requirements for new designs of product may be judged.

As the sophistication and complexity of products and equipment have increased, an increasing degree of reliability consciousness is being exhibited by the product user and reliability is being warranted by the seller for increasing periods. This, of necessity, has created a demand for much more systematic work on designing and organizing manufacturing methods, processes, and test facilities to assure specified product reliability. Raising the level of reliability, as an important factor in product improvement and in the total cost of product manufacture and operation, implies both a decrease in total operation and maintenance cost to the consumer and a decrease in the over-all cost to the producer.

While reliability engineering attacks the problem of gaining maximum equipment and system reliability with available components, involving design optimization (1) as an important ingredient, the "science" of reliability takes the longer range approach through the understanding of basic failure mechanisms in materials and components. Thus research plays an important role in the achievement of reliability through the examination of both the manufacturing environment and use environment which can lead to processing defects or degradation processes. While reliability emphasis has achieved particular recognition in the electronic and aerospace industries, it now attracts increasing attention in manufacturing and processing technology in more conventional consumer product industries. The understanding derived from research on reliability factors provides the possibility for their control; understanding and control yield predictably reliable products and manufacturing methods.

### Avenues for Governmental Action

In order for the United States to maintain its world position of industrial leadership it is essential that it remain in the forefront in discovering,

identifying, and applying new technology for civilian, as well as for military, products. It seems timely to propose that the influence of Government laws, regulations, and policies be utilized to provide a proper climate of incentives by which technology is continuously put into practice in private industry. To insure this is obviously a complex matter.

The greatest infusion of the research and development dollar into the economy has been from the Government. Government involvement in major research programs has been concentrated in either matters of international prestige and security (space, supersonic transport, defense) or in matters which relate to the way people die (pollution, health safety). Perhaps the time has come for Government to exercise its concern in areas of technology which affect the way people live (consumer products, housing, clothing).

The one area of living that has been the traditional concern of the Government is food and here it has met with notable success. One may say that agricultural research supported by the Government for the better part of this century is a prime example of successful research on "manufacturing" processes.

In the sphere of "living economy" some form of Government involvement is needed, short of direct subsidy or control, to enhance the status of applied process research. If there is to be a major modification in the way we fabricate clothes or houses, the Government can be a catalyst either through legislated permissiveness of trade associations, joint research enterprises, promoting interactions and information exchange among unrelated industries and disciplines, or a more activist role through its own purchasing policy, tax policy, or joint Government-industry ventures.

One of the classical ways in which the Government assists the development of new technology is through the mechanism of using its purchasing power. The development of the computer is an example of this. Where the cost of development is large, where an industry is fragmented and/or where the risks are high the normal workings of the marketplace may not permit the application of the latest or the best technology to the fullest or even the research to achieve this technology. The question that needs investigation is whether or not there are some industries, or some opportunities for new industries, in which utilization of its own purchasing power by the Government or its assumption of the role of investment banker would provide a key to successful innovation. There is the possibility that just such an opportunity exists to help solve the central city problem. Indeed, a guarantee of purchase by the Government of mass produced living units, whose design would require and allow the use of assembly line production techniques, might provide a mechanism for introducing new technology into this vital industry. No doubt other

opportunities exist in major socio-technological problem areas which would benefit the Government, the public, and private industry.

In one sense, it is an unfortunate accident of our selection process that with one exception, the examples of innovations presented in the latter part of this paper arise from what we may call "big" industry, although there is no doubt that some of these cases embody innovational inputs from "little" industry and small firms. There is no intent, and no inferences should be drawn from these examples, to relate the size of the industrial enterprise with its technological innovation potential or experience. Indeed, recent studies have argued that small firms, despite their relatively smaller research and development expenditures, contribute significantly to the Nation's inventive and innovative progress. An example of such innovative capacity on the part of small firms is illustrated by the disc-armature type motor example (described later), which well demonstrates the catalyzing effect of processing innovations in one industry leading to product innovations in another industry. We believe, in fact, that the policies of the Federal Government toward "little" industry require special review with the objectives of enhancing and stimulating research and development for the use of new technology and innovation risk incentives. This can be of particular importance for the role of small firms in processing and manufacturing technology.

What about "big" industry? We have seen that only big and progressive industry, in general, can afford, on its own, the extensive investment and involvement required for sound applied research in manufacturing technology. Of the "non-favored," classic industries, only a relatively small number of the larger companies are actively committed to such research. Some of the most significant advances in those "classical" industries, e.g., steel, glass, railroads, construction, have come from abroad. "Big" industry as much as fragmented industry requires incentives to extend and to apply the fruits of its research beyond its traditional boundaries. Such incentives would promote a climate of technological awareness for the national purpose where it is not readily evident (*a priori*) that a given industry or company would derive benefit in the marketplace.

### Challenge of the Future

The producing system is most broadly characterized as a collection of men, materials, and machines that react to the input stimulus of a need of society to produce an output of products or services that will satisfy the need. We have included farther on in our discussion some examples of how a science-based approach to the machine or processes that convert matter and energy in manufacturing industries can lead to new and powerful approaches in production and to major economic and techno-



logical gains. We also discuss in the next section of this paper how the new technologies of computers and information control systems can provide new modes of reaction of people and processes within the producing system. The impact of these information network and control systems will be in the direct control of the manufacturing processes themselves and in the integration of physically separated manufacturing facilities within a given industrial plant and between plants. An equally important impact will be in coupling the manufacturing processes and facilities to the supply and demand network. There exists the significant interface between the manufacturing system and society which the information network and control systems must be designed to bridge if they are to function most effectively, involving such factors as raw materials supplies, labor markets, marketing and distribution functions, etc.

The examples also illustrate how basically motivated scientists, through the technological implications of their own research findings, quite naturally lead to applied research and into later developmental activities. It appears that the "scientific entrepreneurs" or "champions," who derive the intellectual satisfaction and thrill of exploiting the fruits of their own research labors represent one key factor in the science-technology innovation process. This confirms the view of Harvey Brooks, in his paper in *Basic Research and National Goals*, an earlier report by the National Academy of Sciences, of the flow of people trained in university-type research who go into applied science, "which has been one of the characteristic features of American science that has contributed to its vitality."

Research in the area of manufacturing technology will be of major significance for economic well-being both from the viewpoint of individual industrial enterprise and from the national viewpoint. Recent encroachment from foreign sources in manufacturing areas in which United States pride in leadership historically prevailed, with consequent intense price competition with our domestic producers, underscores a fundamental issue for our economic future. Leadership will go with those who innovate in the manufacturing function. Increased emphasis on applied research in processing and manufacturing technologies can provide a powerful science-based approach to sustain our technological and industrial leadership in manufacturing. The Federal Government can be a significant source for providing new avenues of influence and incentives to stimulate such technological awareness in consumer product industries.

### Automatic Process Control

We discuss the automation of processing with particular emphasis on the need to satisfy two basic requirements of the business organization: The first of these is the need to make the process control system compatible

with the automation of other business operations, such as financial reporting and control, the reporting and control of inventories and purchased goods, production scheduling and quality control, and engineering design and release. The second of these is the need for flexibility of equipment utilized in automated processing and the avoidance of large investments in equipment that is limited to the production of particular designs of products.

### ***Role of Systems Design and Computers in Manufacturing Processes***

In the past, most applications of research to industrial activity have been through innovations in materials, processes, and devices. But these simply constitute the building blocks from which a design and manufacturing system is built. An extremely fruitful area for present and future innovation in industry lies in system design at various levels of the structure.

This possibility is enhanced by the advent of third-generation digital computers. The digital computer is oriented towards efficient, rapid, and consistent execution of logical transactions, and thus is an appropriate tool for the implementation of system design. Third-generation digital computers add real-time availability, a faster cycle time, and lower price, to result in a tool so attractive as to justify changes in operating procedure to allow its full use.

In discussing automatic process control, we shall be considering both systems studies (systems synthesis, systems analysis, systems research) and computers—both the technique and the tool. The two innovations combine to provide a large potential impact on manufacturing procedures.

### ***Simple Control Systems in Manufacturing***

In the highly uncentralized manufacturing system commonly found, individual operations are subjected to feedback and feed-forward control . . . but on an individual basis, with little thought of over-all system optimization.

Some examples of simple feedback are shown in Figure 1. The basic action exemplified by Figure 1(a) is seen to apply to design, the manufacturing system, and to the individual manufacturing process. The feedback actions provided by reliability and feasibility studies (for design) and by production scheduling and quality control (for manufacture) have been performed in the past by people through organized activities dedicated to these functions. The control of the individual manufacturing process has been implemented in the past through human operators or through hardwired logical controllers.

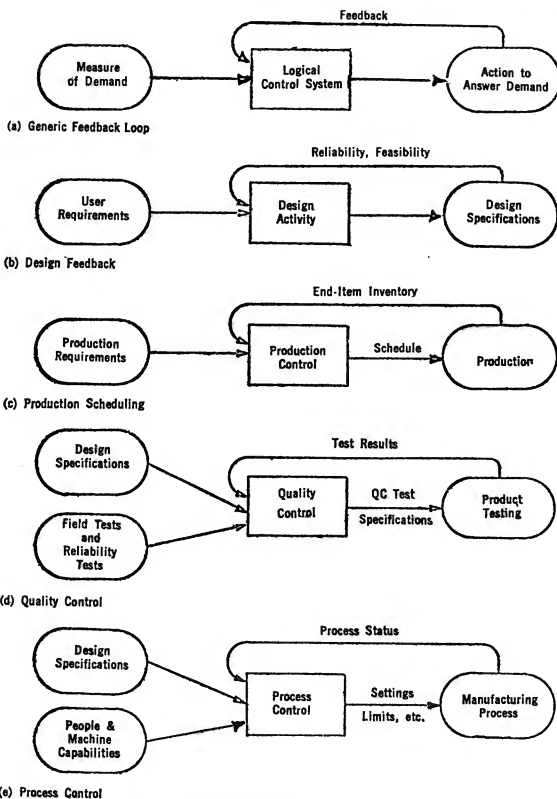


FIGURE 1: Simple feedback loops in manufacturing

The first impact of the computer on these activities has been through the use of off-line computer programs in feasibility studies, reliability, production scheduling, and quality control. This has allowed a more extensive analysis of data, and a more effective use of existing manned activities.

The present and future impact lies in the use of real-time computers. By revising production scheduling, quality control, and similar activities to use timely information from the process a better allocation of men and materials can be made. By replacing inflexible control algorithms hard-wired into manufacturing machinery with process-control systems using digital computers programmed by the user, improvements in the individual processes may be achieved, and flexibility for future change retained. The judgments now made by people in these activities are based on logical processing of pertinent data. By programming this logic into a computer and automatically supplying the pertinent data as it is received from outside the system or upon its generation inside, these judgments can be made more rapidly and consistently.

There are two points of interest here. The first is that although the computer is doing only what a human has previously thought out and instructed it, it can surpass its creator in speed and scope. The speed is already clearly demonstrated. The scope may be exemplified in the classical optimization problem of vitamin mixtures contained in wheat, rice, and eight or ten other foods together with their cost per pound, with a minimum cost diet under constraints of maximum percentages of any one food and minimum time between repeats desired. The human cannot cope with the simultaneous requirements of the problem which is solvable to a true optimum by computer with a proper linear program.

The second point concerns a second-order correction of judgment. In the normal industrial environment, the managers who make these judgments must continuously evaluate their efficacy—and on a real-time basis. The role of supervision is to monitor the judgments of subordinates to see that they are producing the desired results. When corrections are effected, however, the second-order effect is lost and it becomes a first-order judgment. The use of a computer to exercise first-order judgment allows the harder-to-define and more sophisticated second-order judgments of the humans to be given increased attention. More importantly, this judgment monitoring may itself become real-time or close to it.

### ***Process Control***

To what extent is the potential of the real-time computer being implemented in process control? And in what industries?

As Charles Silberman has said recently, automation is having only a slight impact upon people's lives . . . because there isn't very much automation. Those industries whose nature requires "hands-off" control . . . petroleum and chemical, metals, power, nuclear reactors . . . account for two-thirds of all process computers. These applications would require some form of automatic control in any case, and the computer simply replaces other machinery.

In those industries whose operation is primarily fabrication and assembly, a number of data acquisition systems and test sequence control systems have been implemented. Very little has been done in closed-loop direct digital control (DDC). The heavy manufacturing industries, with their large labor forces, are relatively unaffected by computer control at this point in time. This situation is not likely to continue, as the reduced price of computers now makes DDC a competitor to analog controllers. The possibility of adaptive control, hybrid control, and interaction control systems will make the future use of process computers in fabrication as desirable as it has proved to be in the chemicals industry.

Economics of production remains the driving force for technical change. The changes "caused" by new technology are justified by their effect on profit. Competition within a manufacturing industry will cause a general acceptance of extensive automation once the pioneer corporation blazes the trail. We deal, then, with a problem in instability, where we are waiting for the nudge that will move an industry from neutral equilibrium.

How big is that nudge? In continuous processes that were already under automatic control, such as paper mills and oil refineries, a process improvement of 2 percent to 5 percent has been experienced quite generally upon the introduction of process computers. Any product line having more than \$6 million annual volume is then a likely prospect because the DDC system can be paid for out of the savings. In inspection systems for discrete components, economics of scale are again important. In a typical example, the computer is justified on the basis of reduced control cost, for an annual product run of \$7 million value, and results in the avoidance of 60 percent of the control cost for larger production runs. It is hard to imagine a reasonably conceived potential application to any product of annual value greater than \$10 million which could not be cost-justified on the basis of such incremental improvements.

### ***Complex Manufacturing Systems***

Beyond the benefits obtained through improved control algorithms for individual processes, the addition of real-time computers to manufacturing opens up possibilities for interconnecting the individual processes.

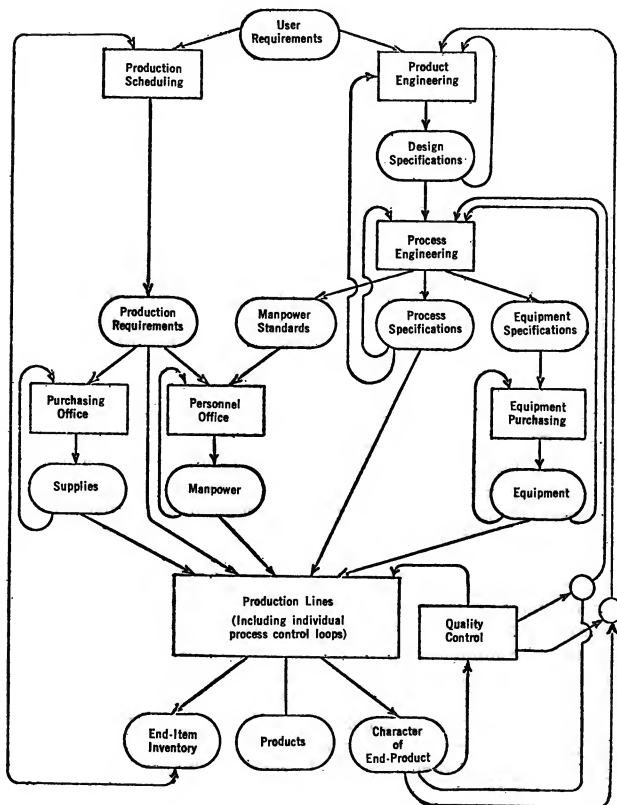


FIGURE 2: Complex manufacturing system

Consider the manufacturing system depicted in Figure 2. In addition to tight feedback loops within individual activities (e.g., purchasing office, process engineering), wide-ranging feedback loops are found, such as that linking the character of the end-product to the process engineering activity.

As individual elements of the design activity, business systems, and production processes are placed in real-time control systems, interconnecting the systems becomes attractive. The cost-justification for a real-time process control computer lies strictly in the control function, but the information generated at the same time can be used in business systems, and by other control systems. For example, the cost justification for a computer-implemented process and equipment catalog lies within the process engineering activity, but the data base so created will also be useful to product engineering and to quality control.

Experience with wide-ranging military command-and-control systems indicates a hazard of a rigid command structure imposed upon real-time computer systems. It appears better to provide real-time interaction between systems on an "information only" basis. Under this concept, an activity would use closed-loop control over its basic function, but would maintain a data bank of historical data and of its present status. Parts of this data bank can be "unlocked," under program control, to other systems. This concept allows controlled communication between subsystems, but avoids the undetected instabilities or "real-time" bugs which plague very wide-reaching rigid control systems.

Corresponding to the manufacturing system, Figure 2, the computerized system at a stage of partial realization may resemble Figure 3. In this figure, computerized business systems are indicated by tape symbols, while a design file and real-time plant control systems are depicted by magnetic drum symbols. The plant executive function includes the routing of all intra-plant communications between control systems, and the funneling of all extra-plant information requests. A real-time executive system links the plant executive and the design file, as well as providing access to all business systems.

In the system depicted, it is not essential that each subsystem have a separate piece of computer hardware associated with it, but it is essential that the operation of each subsystem be independent. At least, each subsystem must have sufficient independence to operate alone during the repair and recovery of any of the other parts of the system which may fail. The system parts must not be subject to a propagation of malfunction . . . a "domino" collapse, as it were.

The real-time nature of the plant control functions will lead to the business systems eventually being placed on a real-time basis, and all the component systems of Figure 3 will be based on random-access files in the completed system. One large time-sharing computer can accommo-

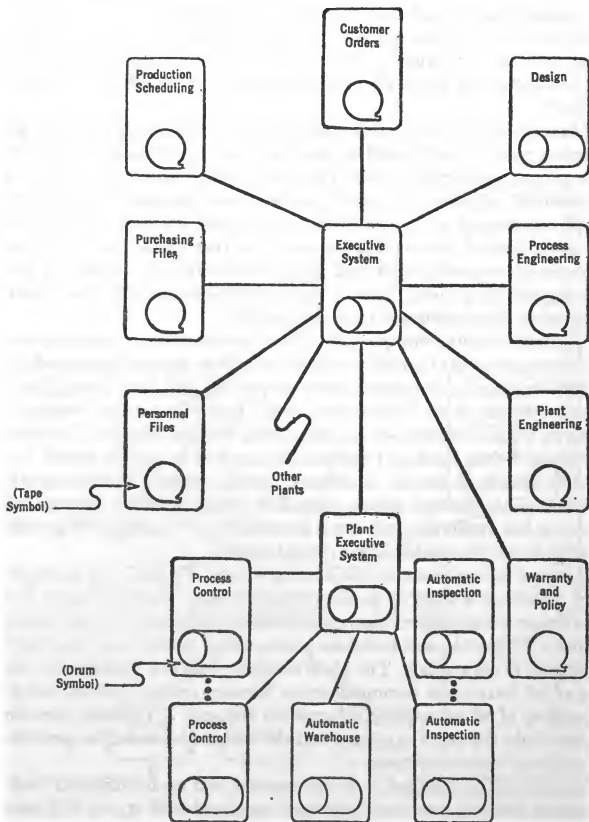


FIGURE 3: Interlinked computer systems for complex manufacturing systems

date all the business systems, with sufficient data resident in a magnetic disc to allow most external requests to be answered within minutes. If necessary for reliability, duplexed central processors may be used.

At the plant end, it is probably that requirement of sub-second response, plus requirements of reliability and backup, will lead to an array of many small computers. The apparent extravagance of many computers may be justified by two factors: the cost of the process input-output interface and the cost of the equipment being controlled. The degree



to which these cost factors will overrule the economies of scale normally observed for computer main frames can only be answered for a specific case . . . no general rule may be applied.

The over-all system organization of Figure 3 resembles a telephone system. The executive system of the time-shared commercial computers acts as a switchboard for the various information calls and commands as they go into and out of the business systems. The plant executive system fills the corresponding role in each plant. The command-and-control functions, the communications functions and business functions share some parts of this system. Thus it may be considered a utility.

The possibilities of real-time computer control place new importance on the system design of the manufacturing system: this design becomes not only an abstract organization chart, but a real information-and-control plan. When the design of a manufacturing system includes a real-time command-and-control network organized as a computer utility, then the full benefits of recent innovations in information technology will be brought to manufacturing.

### Illustrative Examples of Innovation

#### *Example 1: High-Pressure Metal Forming*

One of the most important conversion processes in manufacturing technology is metal forming. There are a wide variety of approaches that are used to form metal alloys into the tremendous variety of shapes that manufactured products require, e.g., machining, casting, metal working, joining, etc. The automobile is almost the perfect example of a product of a manufacturing industry in which practically all known conversion methods for a wide variety of materials in a large and complex number of sequential steps form the basis of the success of the final product. A comparison of the automobile of the 1940's with a current vehicle does not reveal any startling change in the mix of materials. The improved value, capabilities and consumer price appeal of today's models are due much more to the more efficient processing and use of materials than to radically basic differences in design.

The most widely used of the conversion processes are those which form metal by applying controlled mechanical forces and taking advantage of the plasticity of metal in the solid state, e.g., rolling extrusion, deep drawing, punching, etc. Such processes lend themselves to production control (more recently to computer control) and to easy specification of the properties of the final metal part. Most importantly, perhaps, they prove to be the most economical way to fashion metals particularly for large volume production runs.

The mechanical forming of metals is an ancient art starting with the first man to pound a bit of native metal into a useful tool and leading

up to modern machinery. The application of the technique is usually limited by the ductility of the metal from which the part is to be formed. These limitations of ductility, of course, have been appreciated for a long time. Indeed, much alloy development has been directed toward producing materials with much improved combinations of final properties along with the ductility that is required to shape them into useful forms. Metal working machinery has also received attention in order to produce larger and more controlled forming stresses. Nevertheless, with few exceptions, forming processes have not substantially changed in many decades. There has been much concentration on understanding the mechanisms by which metals deform and much effort in alloy development to produce materials with improved formability behavior, but there has been far less effort directed toward forming processes themselves. Similarly, a few new approaches to metal forming have been commercially exploited.

Explosive forming, high-energy rate forming, magnetic forming, thermo-mechanical working, are some approaches receiving current commercial attention, but they still (and perhaps always will) represent highly specialized methods of mechanical shaping for particular materials or special product designs.

In the early part of the 20th century, P. W. Bridgman of Harvard University observed that under large hydrostatic pressures in the range of 100,000 psi and above the mechanical ductility of many metals was greatly increased. Bridgman himself foresaw the implications of this discovery for engineering application to the forming of metals, and such possibilities later intrigued many people to explore the utilization of this phenomenon. It is interesting to observe that many of the reported attempts were directed to materials which could not normally be worked by conventional techniques. The refractory metals, such as molybdenum and tungsten, received special attention. Indeed, much of the past investigations appeared to be stimulated from a product (rather than processing) point of view, in an attempt to make new structure from a new material which had not been possible previously.

The Western Electric Engineering Research Center has been involved during the past few years in the examination of a broad area of metal forming from such a process point of view. Western Electric is a large converter of metals; although the mechanical devices they make, such as relays, are relatively small, they are made in extremely large numbers with demanding precision requirements. For example, each telephone hand set contains several hundred parts and many of them are metal parts of high precision. The objective of the Western Electric program was to seek out new generalized approaches for metal forming which might have production advantages either in increasing the variety of parts that could be formed, improving the quality of parts, or providing

economic advantage—i.e., it was a program of research on processing methods and technology. Fred Fuchs and his associates at Western Electric were aware of Bridgman's earlier results and decided to reinvestigate the potential of hydrostatic pressure forming, but with a different orientation. They realized that there could be significant economic advantages if one could produce significantly increased ductility in those materials, such as copper and steel, which are normally considered quite ductile and easy to form.

The research program has proven to be exceedingly profitable. It has been shown that at relatively low hydrostatic pressures, about 100,000 psi, the increase in ductility of copper permits very large deformations without fracture. A process, based on this technique, has been developed by which a connector (used in large quantities in the manufacture of broad band telecommunications systems) is cold formed in a single operation from a tube. This replaces a relatively expensive electroforming technique.

Another challenge was the development of production tooling which would permit a reasonable production rate. The tooling must be able to contain fluid pressures in the neighborhood of 100,000 psi and yet the sealing operation must be rapid and reproducible. This required the successful development of a structure which permits rapid sealing of hydrostatic fluids over a very large number of cycles.

The hydrostatic pressures process has also been applied to the forming of steel. A relay cover can be formed from a mild steel disc in a single drawing operation as compared to a series of operations requiring four successive draws with intermediate anneals with the use of conventional deep drawing techniques. The techniques have also been extended to even higher pressures.

This is an intriguing example of how a science-based, systematic approach to an important processing function has permitted the utilization of existing science to an established, conventional process, thereby producing a new and extremely broad process technology that is having a very large impact in the manufacturing operations of at least one industrial organization. It also nicely demonstrates the "feedback" cycle in the science-technology transfer process, since new areas of research have been stimulated by the development. The large increases in ductility at large hydrostatic pressures are not at all well understood and the fatigue behavior of steels at high pressures remains an unexplored area. Obviously, applied research for improvement of manufacturing and processing technology, in the proper hands evokes considerable intellectual stimulation and satisfaction.

### ***Example 2: Titanium Carbide Cutting Tools***

Machining is another key process indispensable to manufacturing industries. In a company such as the Ford Motor Company, millions of

pieces are subjected to machining operations, tremendous numbers of man-machine hours are involved, with an enormous capital investment in machining and machine tool facilities and equipment. Any improvement in machine tool performance and life, in speed of machinability operations, in reduction of processing steps or in handling of parts obviously could result in dramatic improvements in cost and profitability. The following example illustrates how a material development, initiating from objectives quite foreign to machinability improvement, indeed provided the Ford Motor Company with an innovational opportunity for spectacular economic gains in its over-all machining operations on manufacturing lines.

According to the historical account of cemented carbide developments, cemented titanium carbide-base materials were first introduced commercially in 1930 and represented the first carbide tools for the high-speed cutting of steel. At about the same time, the steel cutting grades of tungsten carbide containing titanium carbide and tantalum carbide additions, were also made available (1931) and subsequently became the predominant compositions for steel machining. The lack of more widespread use of these early cemented titanium carbide grades is attributed to their low strengths, which were only about 50–60 percent that of tungsten carbide-cobalt compositions.

In the decade following World War II, the cemented titanium carbides again received considerable attention, although during this period they were being developed as a cement type material for possible high-temperature structure applications. It was hopefully anticipated that cermets (ceramic-metal composites) would provide high-temperature materials that would combine the desirable properties of high temperature strength and toughness of each of the constituents in a single material. The major effort in cermets was directed to the development of titanium carbide base materials; however, in the final analysis, a lack of adequate impact resistance prohibited their use in high temperature applications. It is interesting to note that, as with the early cemented titanium carbide tools, the later cemented titanium carbide "cermets" also exhibited strengths only about 50–60 percent that of tungsten carbide-cobalt compositions.

The Scientific Laboratory of the Ford Motor Company was one of numerous organizations active in the titanium carbide cermet field in the middle 1950's. The original motivation for their research program was the hopeful development of improved high temperature materials for gas turbines for vehicular transportation. Recognizing, however, that the strength and fracture resistance of cemented carbides were intimately controlled by microstructural features, i.e., carbide particles size, distribution of carbide phase, mean free path between carbide particles, etc., a fundamental approach was undertaken by M. Humenik, Jr. to

determine the important factors influencing the microstructure in sintered carbide systems. Fortunately, it was recognized early that wettability considerations between the liquid and solid phases during the liquid phase sintering stage might be an important key to the control of the microstructure, and a research emphasis was placed on the study of factors influencing wettability in bonded carbide compositions. This research program led to the finding that the wetting of titanium carbide by nickel could be enhanced by the addition of molybdenum to the nickel phase. The effect of the improved wetting resulted in a marked refinement in carbide grain size. The research also led to a significant contribution to the understanding of the science underlying sintering phenomena in carbide systems by elucidating proper mechanisms for grain growth of the carbide phase. Sufficient understanding of the basic phenomena now exists to predict the carbide and composition changes that occur during sintering.

The refinement in carbide grain size and over-all improvement in cemented carbide microstructure, as a consequence of the effects of molybdenum on wettability, resulted in a substantial increase in the strength and hardness of the titanium carbide cermets. Whereas the strength of earlier titanium carbide cermets were only 50–60 percent of commercial tungsten carbide-cobalt composition grades, comparable strengths were obtained with the newer compositions.

The microstructural features and the physical and hardness properties of these new titanium carbide cermets led the research scientists to speculate on the potential of these materials for cutting tool application. The use of titanium carbide as an addition to tungsten carbide-cobalt cutting tools in order to provide cratering resistance was well known, and was one of the significant advances which permitted tungsten carbide cutting tools to be so effectively used for machining steels. Since titanium carbide is inherently harder than tungsten carbide, it was reasoned that in cutting tools, wherein the carbide phase is essentially all titanium carbide, further improvements in cratering resistance and tool wear could be attained. Laboratory machinability tests quickly demonstrated the realization of these expectations, which were subsequently confirmed in more detail in a variety of production machining tests. A four- to sevenfold improvement in tool life and significant increases in permissible cutting speeds revealed the marked superiority of the titanium carbide tool composition as compared to standard commercial tungsten carbide tool grades.

The success of these early machinability results, the exciting promise of their commercial and manufacturing significance and the encouraging interest of the operating divisions of the company in their utilization led to a major program for the development and optimization of nickel bonded titanium carbide (containing molybdenum as an addition) cut-

ting tool materials. In 1959, a short four to five years after the initial research was begun, the Ford Motor Company announced the development of these new cemented titanium carbide cutting tools as a superior replacement for tungsten carbide grades. Since then a whole series of titanium carbide tool grades have been developed for use for rough, semi-finish and finish steel machining operations. More recently the evaluation of titanium carbide tools for machining the high carbon-iron alloys (nodular, malleable, cast iron) has been initiated. It has already been established that superior surface finish is obtained using titanium carbide tools and that at high cutting speeds greater tool life is also obtained.

One of the interesting features of this technological innovation was the fact that Humenik and his colleagues who were involved in the original research efforts carried the whole program through to its ultimate developmental stages. The science-technology transfer process was most rapidly achieved by scientists with entrepreneurial instincts. More and more, case studies seem to indicate that "scientific entrepreneurs" or technologically motivated "champions" represent a key factor in the translation of science to technology.

Some idea of the important economic significance of the above titanium carbide cutting tool innovation is evident from the fact that the Ford Motor Company has already substituted these new tool compositions to replace conventional tungsten carbide tool grades in all semi-finish and finish steel machining operations and is currently introducing the tougher grades of titanium carbides in rough machining operations. In terms of increased cutting speeds and over-all performance, decreased tool costs, decreased labor costs, and decreases in machine and tooling costs, it is estimated that the Ford Motor Company is currently saving about \$3.5 million a year in manufacturing operations. It is confidently projected that annual savings in machining productivity and capital investment will achieve even more dramatic gains over the next few years.

So we see how, in one instance, a material innovation from research, with origins and motivation for different objectives, is producing a major economic impact upon a long established field of manufacturing technology. It is not too optimistic to project that similar science-based approaches to machining and metal removal operations in general could produce other important innovational opportunities in these fields. When one considers that the metal removal industry involves a yearly level of about \$34 billion, representing about 5 percent of our gross national product, the economic gains to be derived from processing innovations in this field of manufacturing is self-evident.

### ***Example 3: Corfam and Nylon—Synthetic Polymers***

*"Corfam" poromeric shoe upper material.* "Corfam" started as a dream back in 1910, but direct work on shoe materials, out of which

"Corfam" grew, did not start until 1938. In the succeeding decade, research produced a variety of materials which had some of the required properties—breathability and durability—but which lacked the vital aesthetics. By 1958, however, the general concept of a satisfactory material had been delineated. The substrate would be a needled, non-woven web of polyester fibers, which would be impregnated and then surface-coated with a moisture permeable polymer. A scientific breakthrough largely resolved the basic chemistry needed to achieve this permeable polymer.

However, real challenges for innovation remained in mechanical and engineering, as well as chemical, areas before a single product having a combination of all the needed qualities, each to the desired degree, could be produced commercially. A series of experimental semiworks and pilot plants, built to optimize and refine such qualities as color, finish, texture, surface characteristics, breatheability, flexibility, durability, were manned by teams of chemists, engineers, physicists, and even mathematicians. Research and development for "Corfam" has required over 200 man-years, with much of this effort devoted to perfecting the final product.

Succeeding development facilities became more nearly continuous, because as with most sheet materials and "Corfam" is no exception, both the rate of motion of webs and their widths critically affect their properties and process yields. Since conventional web forming equipment was found inadequate, more sensitive and sophisticated devices had to be designed and built.

Developing a process to impregnate the web with a polymer required ingenious application of chemical engineering principles. The finished structure must be permeable, possess long flex life and show the peculiar bending modulus characteristics that give little resistance at large radii but very large resistance at small radii. This was accomplished by interlocking, in the same three-dimensional space, two independent frameworks: the original fibrous web and a polymeric, skeletal structure. The polymer is deposited in the fibrous web so that it does not physically adhere to the web fibers. The polymer, initially in mobile form is diffused into the web and immobilized by an environmental change. The polymer carrier is removed without distorting the skeletal structure by counter-current extraction requiring closely controlled, simultaneous mass and heat transfers.

The permeable, polymeric surface coating must be mobile at the time of deposition but immobilized reproducibly immediately thereafter so that pore structure, flex life, and moduli characteristics are maintained at predictable values. To accomplish this the mobile polymer is handled in an area of its phase diagram where very small changes in temperature or composition cause large changes in the viscosity of the

polymer-solvent-diluent mix. These small changes are then made at the proper instant by precisely controlling heat and material balances. Further major material and mass transfers produce a permanently tough and yet resilient structure. The coating step constituted a wholly new unit process which required the development of unique processing equipment.

After the coating technology had been reasonably well developed, the nearly dormant napped product received a shot in the arm when a chemist stumbled onto the secret of generating very fine naps with bright intense colors. Thus the product that had provided much of the original incentive for a pilot plant but had then lost priority re-emerged as an attractive running mate for the coated material. Now both types of shoe upper material are commercial realities.

The broad background of Du Pont's engineers and scientists, built upon years of experience in applying the general principles of technology and engineering to the design and operation of new plant equipment, contributed importantly to the success of this venture.

4. *Nylon*. In 1928 Du Pont initiated long-range, scouting research in the little-known field of polymerization—the union of small molecules to form larger ones such as those found in rubber, cotton, and wool.

Early work produced fibers that were far too easily destroyed by water or heat. But they encouraged the search for better ones that would have properties satisfactory for textile purposes. Then in 1938, du Pont announced that it had found a family of polymers, the nylons, from which surprisingly strong and elastic fibers could be spun and from which superior bristles could be made.

But at this stage the battle had scarcely begun. The tasks of developing laboratory processes for the intermediates, the polymer itself, and nylon yarn, and of acquiring on a semiworks scale, the chemical and engineering data for the construction and operation of a large-scale plant were enormous. In order to reduce to a minimum the time between test tube and counter a large force of highly competent chemists and engineers was assembled from Du Pont's Engineering and Rayon Departments. No previous experience with the types of materials and processes involved existed in the company. Nylon polymer had properties entirely different from those of any previous synthetic material and spinning yarn from molten polymer was entirely different from spinning either cellulose acetate or viscose rayon. For example, special pumps for operation at 285°C with small clearances and no lubrication other than the polymer itself, had to be designed. However, except for size, the first commercial plant, which went into operation early in 1941, was very much like the semiworks plant in all details. And the second plant was virtually a duplicate of the first.



But before nylon could reach the ultimate user, many problems associated with the conversion of the yarn into finished fabrics had to be solved—problems of which most chemists and engineers had little knowledge. Yarns of various diameters and lusters had to be developed and the good qualities of the new yarns had to be confirmed by actual wear tests. Technical service men, versed in every phase of basic textile technology, went into the field and showed textile mills how to use nylon yarn—for example, how to pre-board hosiery so that it would maintain its original shape. Basic information for all of these steps was acquired only by painstaking experimentation. The ingenuity and talent of some of the best chemists and chemical engineers within the company were devoted to the applied research needed to bring the commercialization of nylon to a very successful conclusion.

Even after all the initial problems were solved, applied research on manufacturing processes could not cease. In order to remain competitive on the one hand and increase volume on the other, ways to reduce cost of manufacture of nylon yarns and filaments have been aggressively sought. Continued applied research over the years has improved yields and reduced investments for the intermediates, polymer and yarn, so that the price of nylon has dropped about 40 percent from the price at which it was introduced 25 years ago.

#### ***Example 4: Electrocoating—A New Painting System***

In the mid-1950's corrosion of automobile bodies emerged as a major problem confronting the automotive industry. Its occurrence coincided with the rise in popularity of the use of salt to control snow and ice. There is an increasing trend, on the part of local communities, cities, and highway officials favoring the utilization of salt to minimize or eliminate plowing snow as a way of saving snow-removal monies. While corrosion of exterior trim became troublesome, the most severe problem was encountered with corrosion starting from the inside of the body and progressing outwardly, resulting in metal perforation. Aggressive programs on the part of the automotive industry involving design for corrosion control, improved electroplating systems, better paint systems, increased attention to quality control and inspection, etc., have resulted in substantial reduction of corrosion problems. One of the most effective methods for control of corrosion of current car models (starting about 1960) is the widespread use of galvanized zinc sacrificial coatings and zinc-rich primer paints for steel underbodies.

Much of the difficulties associated with the utilization of potential corrosion control solutions is inherently related to the complex manufacturing operations involving the assembly of many small parts into subassemblies and final consolidation into a complete car body, involving numerous welding and forming operations. The electrocoating system for painting

developed by the Ford Motor Company is a striking example of how introduction of a new processing and manufacturing technology can provide solutions to difficult technical problems in the service performance of finished products. The story essentially has its origin in the recognition by G. Brewer and G. Burnside, chemical engineers in the Manufacturing Development Office of Ford, of the applicability of the principles of electrophoresis for development of a painting process using water-based paint systems. It illustrates well, again, how old science can be used to develop a new manufacturing process.

The process takes advantage of certain of the fundamental properties of colloids. In any colloidal dispersion, an electric charge on each individual particle causes the particles to repel each other, tending to offset the effects of gravity and to prevent the particles from settling or agglomerating. When a suitable voltage is imposed on a colloidal dispersion, the dispersed particles are attracted to the electrodes of the opposite charge. Upon contact with the electrode, as a result of emulsion and electrochemical phenomena the particles give up their electric charges and coagulate as a converted deposit on the surface of the electrode. When a pigment is incorporated in the colloidal system to produce a paint, the same film-forming phenomenon is observed.

In actual process practice, water is primarily used as the carrier of colloidal materials, the actual work piece immersed in the bath is used as one electrode and the steel dip tank or auxiliary electrodes serve to complete the electrical circuit. As the deposited paint particles build up a certain film thickness, the paint film acts as an insulator and forces additional particles to paint uncoated or thinly coated areas. In this way a uniform coating is deposited on all surfaces of the work piece in contact with the bath. The paint is deposited as an essentially dry film, since water is rejected from the coated layer as it forms, probably by electroendosmosis. The process is completed by rinsing with water to remove adhering droplets of unconverted paint from the bath, followed by a conventional baking cycle.

Other paint processes (brushing, spraying, dipping, etc.) are based on the principle of transferring a given paint from a container to the part to be coated, without changing the composition of the non-volatile portions. The electrocoating process, on the other hand, distinguishes sharply between the bath composition and the film composition. Certain bath components such as solvents and particular ions are essentially not removed from the bath at all, whereas other components may be removed from the bath in proportions different from their relative ratios in the bath. Proper bath composition is maintained, therefore, by feeding into the bath during operation only those substances that are removed and feeding them in the exact proportions in which they are removed. Paint utilization is of the order of 90 percent efficient.

Some of the important advantages of electrocoating over conventional painting systems include: greater throwing power, high paint efficiencies, uniform film thickness automatically controlled, ability to coat recesses and other interior areas difficult to reach manually, ready formulation of paint systems for different coating purposes, low maintenance costs, absence of solvent reflux action (redissolution of deposited paint films by solvent vapors). These processing advantages only supplement the chief advantage of the electrocoating process in providing a direct production line method for protection against corrosion of exterior and interior surfaces. This electrocoating process is now in operation for the prime coating of bodies of two vehicle models of the Ford Motor Company and for painting wheels. It has been extensively introduced into the Ford Motor Company plants abroad. Over 25 million electrocoated wheels are in use and more than one million vehicle bodies have been electrocoated to date. More recently a new one coat paint system has been developed which lends itself to electrocoating which, on the basis of its appearance and corrosion protection characteristics, has a high potential for replacing current two coat primer and finish coat processes.

From the viewpoint of the science-technology transfer process, there are several intriguing features about this electrocoating development. First, the recognition and receptivity of engineering personnel to the potential of a known scientific phenomenon for generating a new manufacturing process innovation. Second, the willingness to "design" a completely new manufacturing process to solve a major technical problem, rather than just rely upon attempts to improve existing conventional processes. Third, the remarkably short time involved from concept recognition through laboratory demonstration to pilot line feasibility with production installations; the entire cycle was covered within a three-year period. Fourth, the unquestioned role of "champions" who had faith in the process and the determination to promote it. Fifth, the ready willingness and acceptance of top management to support the innovation potential. And finally, the conduct of all phases of the work and monitoring of production installations by one group.

#### ***Example 5: Radiation Curing of Polymers***

As part of a fundamental research program on molecular structure of polymers and polymerization mechanisms, a study of the effects of ionizing radiation on polymer systems was initiated in 1956 in the Science Laboratory of the Ford Motor Company. A background of information about the field existed from prior work in Atomic Energy Commission Laboratories and elsewhere, both gamma rays and high-energy electrons were used. Based studies conducted by W. Burlant at Ford revealed that certain unsaturated molecules are hardened rapidly, without heat, when exposed to such radiation and that a wide range of physical and me-

chanical properties could be developed by suitable "tailoring" of the polymer structure. It became clear that radiation-induced polymerization might provide the basis for developing an entirely new painting system with the economic advantage of elimination of lengthy high-temperature oven-curing steps involved in conventional painting of automotive vehicle components.

A two-pronged program was begun in 1961 to investigate the modifications required to transform laboratory findings into useful coating systems: development of suitable paints and the design and fabrication of commercially feasible electron sources were undertaken. The personnel involved included chemists, physicists, electrical engineers, and paint chemists, and once again it may be noted that the original leading research personnel were the "translating" agents for the development.

Polymer structures exhibiting maximum sensitivity to radiation, and with rheological and weathering properties required for optimum performance, were developed. Important chemical parameters included optimum incorporation of radiant resistant groups in the polymer molecule, molecular weight proper flow properties and alterations of chemical configuration to ensure adhesion durability. The result is a coating that cures in three seconds and at room temperature. Painting on various substrates was investigated, e.g., metals, wood, rubber. The radiation passes through the coating, and in the case of plastic and wood materials, penetrates the substrate slightly, creating reactive sites in the substrate. The sites allow crosslinking or bonding to take place between the coating and substrate, and ensures outstanding adhesion. The resulting finish has the properties of a baked enamel—it will not peel, crack, or blister. The hardness and flexibility of the coating can be varied by controlling both the degree unsaturation in the paint and the extent of crosslinking which occurs during cure.

A suitable paint is only part of the radiation process: inexpensive, rugged electron beam equipment is required to ensure production use of this technique. Commercially available 1 MeV electron sources were unsuitable for continuous industrial use, and prohibitively expensive. From the equipment-development program also begun in 1961, there were developed unique electron guns capable of providing an inexpensive, high-intensity electron beam. A new technology was developed at Ford to generate "low-energy" (300 kV) electron beams. Equipment of this kind makes radiation processing commercially attractive because:

1. Beam penetration of the low-energy electrons is only a few thousandths of an inch (that's about the desired thickness of paint films) so polymerization of these films on radiation-sensitive materials such as wood and plastics now is possible.
2. The chemical reactions are initiated more efficiently than with higher-energy electrons.

3. Equipment is simple, powered by a low-priced, easily available transformer, several fold cheaper than previously available larger units; additionally, shielding requirements are minimized.

Applications of radiation-cured paint systems in Ford operations include:

1. Steering columns, where painting after assembly (requiring rubber and plastic fittings) provides savings in handling.

2. Plastic components, where electron-initiated curing in 3 seconds, compared with 45 minutes required with oven-cured paints, speeds up the assembly line.

3. Inks for phenolic printed circuit boards, where the low-temperature cure eliminates heat distortion of the plastic, thereby improving quality control of the product.

All of these are in the pilot evaluation stage.

Extra-automotive applications of the process were also pursued. Factory prefinishing of wood siding is an example of a process that benefits uniquely from all three important features of radiation curing—low temperature, speed, and increased adhesion between paint and substrate. The Boise Cascade Corporation was licensed on a royalty basis in 1965 to use Ford's process to precoat plywood siding. A Ford-designed two-gun pilot plant has been in operation for some time at Boise; this installation is capable of curing 20 million square feet a year, in two shifts. The pilot plant engineering and commercial quantities of the paint were provided by Ford Industrial Chemicals and Products Division.

Radiation-cured paints and polymers represent an innovation in which materials, process, and design improvements had to proceed on a unified basis for optimization of the development into a manufacturing process. Its origins were in research and the full development was carried out by the original research group. It also illustrates the advantages of innovating in an environment which encourages the exploitation of opportunities for diversified operations, not necessarily related to standard products of the company.

### ***Example 6: The Basic Oxygen Furnace in Steel Making***

In the past 25 years there have been several important developments in the steel industry in which the United States has not been the original or the leading innovator. These developments are the basic oxygen furnace, vacuum degassing to remove hydrogen, continuous casting, and certain aspects of blast furnace technology. The fact that the United States was not the leader in these innovations has been disturbing to the executives of the industry as well as its critics. In this section our attention will be directed toward the basic oxygen, although the analysis and conclusions we draw are applicable to the other areas mentioned above.

The idea of injecting virtually pure oxygen by means of a water cooled jet onto the top surface of a molten bath of steel in order to oxidize carbon

in the bath is indeed simple in concept. At least two of the advantages of using oxygen rather than air were readily predictable—a lower nitrogen content of the steel and the ability to charge more scrap. The former arises because no nitrogen is introduced in the bath from the virtually pure oxygen during oxidation of the carbon; the latter is accomplished because the heat which is necessary to raise the temperature of the nitrogen in the air when air is employed to oxidize the carbon can be used to melt additional scrap in the furnace. Low nitrogen content is important from the standpoint of obtaining better mechanical properties in the steel than is obtained from the Bessemer (which uses air). In order for the basic oxygen furnace to replace the open hearth, as high a percentage of steel scrap as possible is essential.

A vital part of the development of the use of virtually pure oxygen to oxidize the carbon out of a steel bath was the availability of cheap oxygen in tonnage quantities. Innovations which were developed during World War II for the liquification of air made this possible. Indeed, at least one prominent chemist in the United States, before the development of the basic oxygen furnace, predicted that the availability of cheap oxygen made possible by wartime inventions would revolutionize the steel industry without his knowing the exact mechanism by which this would and could be accomplished.

The stage thus was set for the innovation to take place. The principles of oxidation were well known, the disadvantages of using air to oxidize carbon in the Bessemer were well known, the advantages of using pure oxygen were readily predictable, and although tonnage quantities of oxygen at low cost were not readily available the techniques had been developed.

The practical demonstration of the innovation, however, did not take place in the United States but in Austria. The reason that it took place there was a simple one—additional steelmaking capacity was needed and there was not enough money to buy an open hearth shop.

The combination of some basic conceptual and experimental work on the process by a professor in Switzerland and of development work in Austria by a company with limited resources resulted in the plant demonstration of the practicality of the method. Other more sophisticated facilities to accomplish the same ends existed elsewhere in Europe and in the United States.

Though we can make the case that success required intelligent, creative, and dedicated individuals, and these were critical, it is hard to make the case that such individuals did not exist in the United States or that they were not in or available to the steel industry. Indeed, as related above, the consistent pattern of successful process innovations coming from abroad is indicative of factors other than the lack of an adequate scientific and engineering capability in the United States.

The pattern of the development of the basic oxygen furnace in the United States once it was demonstrated to be practical sheds further light on the subject. The first applications in the United States were in the smaller steel companies.

The experience of the United States related above in this particular industry is one which is apt to be repeated with increasing frequency in the future. If the United States chooses to rely heavily on innovation and on new technology to maintain its position of industrial leadership in the world, then Government and industry together must adopt policies that more certainly ensure that this comes about. It is on this premise that we make the following observations.

First, the vast majority of the efforts in the steel industry of the United States in process research was directed toward the improvement of existing processes. This policy has paid many dividends and was a prudent one, considering the circumstances in which the companies and individuals within the companies found themselves. The high total costs and the high risks involved in the conception and development of new innovations in processes, the purchase of new equipment, and the losses associated with scrapping equipment which still has useful life made it more attractive for the industry to do research and development work aimed at refining existing processes rather than to invest in new ones. Hindsight may provide information which demonstrates that existing resources in money and talent might have been spent in better directions. This, however, does not solve the problem. The roots go deeper than the policies of any company or the selection of the personnel who were in positions of authority.

Second, the efforts of the departments and agencies of the Federal Government have been directed largely toward national goals in the defense, space, health, safety, and anti-pollution fields—with a resulting diversion of the available talent and energy toward these more fashionable fields rather than in classical, basic industries. Indeed, the tendency has been for the more audacious to enter into these new fields rather than to enter the classic industries. This has important consequences for the future.

The general conclusion to be drawn from experiences related to research, development, and innovation in processing in the steel industry over the past 25 years is that it should be the policy of the Federal Government to offer appropriate incentives to the steel and other basic industries, for stimulating the development and use of new technology in these basic, classical industries in this country.

#### *Example 7: Zone Refining and the Transistor*

The transistor is well known. It was discovered late in 1947 by Bell Laboratories' scientists Bardeen, Brattain, and Shockley, for which they later received the Nobel Award for Physics.

Zone refining is a new and simple method of purifying materials. It was invented in 1951 by W. G. Pfann, a research metallurgist at Bell Laboratories.

Zone refining did not make discovery of the transistor possible. (After all zone refining was invented four years later.) It did make manufacture of the transistor feasible.

The transistor effect—that is, that certain semiconducting elements, germanium and silicon, could, if properly treated and if pure enough, amplify electrical signals—depended for success on levels of purity hitherto barely achieved by man. The application of zone refining at once resolved these purity problems by producing the purest germanium ever known. It eliminated the serious manufacturing problem of lack of control of the basic raw material. It was soon followed by a sharp rise in transistor production.

Zone refining simply is to place a long bar of a fairly pure material, such as germanium, in a tube of graphite, and by focused heaters, to melt short lengths of the bar, and to slowly move these melted lengths (or zones) along the bar, to the end. Impurities tend to remain in the liquid and so, as these molten zones one after another, move to the end of the bar, they carry impurities with them, thereby purifying the remainder of the bar.

This simple idea was unrealized until Pfann introduced it in 1951. Since then it has not only helped to kick off the transistor revolution, but has spread to many other fields—metal physics and technology—chemicals, inorganic, and organic.

The purity of zone-refined germanium is fantastic. It can be expressed in numbers by saying that there is less than one part of harmful impurity in 10 billion parts of germanium. (Expressed in another way, this corresponds to about one grain of salt in a freight car load of sugar.) The harmful impurities are elements that affect the electrical conductivity of the germanium, for example, antimony, phosphorus, and boron, and elements that act as traps for electrons in the germanium, for example, copper, and nickel.

Subsequently, silicon became a favored semiconducting element for transistors. Zone refining was applied to silicon also. However, silicon is a very reactive element and melts at a very high temperature, over 2500° F, and it reacted chemically with all known container materials and so became contaminated.

This problem fortunately was solved by the invention of the “floating zone” technique of zone refining. In this technique a vertical rod of silicon is clamped at either end, and an inductively heated molten zone is caused to move along it repeatedly. If the zone is kept short enough, its own surface tension holds it in place.



The floating zone technique was invented at Bell Laboratories by H. C. Theuerer (although later discovered independently by Keck and Golay of the Signal Corps Laboratories, New Jersey and by Emeis in Germany). The purity achieved in float zoned silicon was even greater than that for germanium, approximately one part of harmful impurity in 100 billion parts of germanium. This technique also has become standard commercial practice for the manufacture of silicon transistors.

As mentioned above, the use of zone refining, either with containers or as the floating zone technique, has revolutionized thinking about purity in many other fields than the transistor. For example:

- a. About one third of the elements comprising the physical world have been raised to the highest purity ever achieved.
- b. Several hundred other semiconductors and inorganic chemical compounds have been so purified.
- c. Several hundred organic compounds have been so purified.

The end is not in sight.

The point is, that a transistor-stimulated invention (which had much earlier links to pure research at Bell Laboratories not described here and not connected to the transistor) has not only helped to advance the transistor revolution, but since has had an impressive influence, both practical and fundamental, in many other fields.

#### ***Example 8: Microelectronics: Its Dependence on Advanced Manufacturing Technology***

Probably the most significant advance of the 1960's in electronics has been the development of microminiature integrated circuits—specifically in the so-called monolithic silicon form. As is now widely known, in a tiny rectangular slab of single-crystal silicon typically 0.050 inch on a side, one can create a complete circuit containing hundreds of elements which can execute a complex digital logic function or amplify a high-frequency communications signal.

While initially the laboratory dream may have been to attain extremely small size for those specialized applications requiring high component densities, several additional achievements of equal or greater importance also have evolved. First, because the interconnections among elements comprising the microcircuit are internal to the structure, once made, they maintain their integrity with a high order of predictability. For this and other reasons, the over-all reliability of a microcircuit has been increased one or two orders of magnitude over that of its equivalent in discrete component form. This factor is central to the success of microelectronics in military and space systems.

Secondly, the ability to fabricate extremely small circuit elements in close proximity has resulted more recently in the attainment of very high-speed circuits (both analog and digital) that would otherwise be inhibited by propagation-time limitations.

But perhaps most noteworthy is that these monolithic silicon microcircuits and their variants are being realized not as premium-cost elements, but as competitively priced items outstripping other circuit forms in cost-effectiveness. This accounts for their gradual appearance in industrial electronic systems where per unit costs are critical, and still more recently, in consumer electronics products. In the 1970's, substantially all new data-processing equipment and many consumer products, such as radios and television, will incorporate microcircuits in large numbers.

The ability to mass-produce these devices at an economic level must be credited not only to advances in the application of basic phenomena at the laboratory level, but to equally impressive gains in manufacturing and assembly technology which followed closely on the heels of results produced by the research and development community. To gain an appreciation for the complexities involved, to produce each batch of circuits requires between 100 and 150 process steps. The processing itself involves such steps as epitaxial growth, temperature-controlled diffusions of impurities into polished silicon wafers accurately sliced from a grown ingot, oxidic growth for surface passivation, and vacuum aluminization for forming interconnections among the circuit elements. The accuracies in registry and the fineness of lines are measured in fractions of a mil.

It is also noted that the diffusion must be highly localized on the surface of the silicon wafer; this is accomplished using masks formed by the chemical removal of the grown oxide in selected areas. Because a number of diffusions involving differing impurities must occur, oxide layers are etched back and regrown several times during the formation of the circuit.

The techniques of physical chemistry employed in the process are aided also by photographic methods. Localized etching of the oxidized layer is accomplished by depositing directly on the surface a photolithographic "resist;" when exposed to light (according to the desired pattern) and developed, this photosensitive material polymerizes into an etchant-resistant film. The optical patterns to be impressed on the coated surfaces during exposure are derived by photographing dimensionally-stable plastic cutouts several feet in dimension, and suitably photo-reducing these to obtain final contact plates.

A single silicon wafer typically has several hundred identical microcircuits produced in it simultaneously which are separated in a later stage of processing. The contact plates through which the diffusion patterns are transferred to the photo-resist on the silicon surface must contain not one but hundreds of repetitive patterns, one for each microcircuit. The photo-reduction must be, therefore, accompanied by a "step and repeat" process or equivalent so that the multiple images can be realized.

Electrical testing is performed on the circuit even prior to separation of individual chips from the wafer. This involves the contacting of the

silicon surface by an assemblage of precisely-spaced needle-like probes, connected to a complex electrical tester programmed to sequence rapidly through a variety of tests. Alignment of these probes to the contact points requires the use of high-power microscopes.

After the circuit chip has been separated from its neighbors by scribing and breaking apart the wafer, it is placed into its final package of typical dimensions  $\frac{1}{8}$ -inch on a side. External connections from the chip to the package leads are made by fine wires thermo-compression bonded to appropriate areas on the surface of the silicon chip.

In the past three years, these relatively exotic techniques have come out of the laboratory and are being applied now using semi-skilled labor in production facilities. The industry presently is producing circuits at an approximate annual rate of 20 million units; in the 1970's this level will be upped by a factor of perhaps 20. The cost per function will reduce drastically in the succeeding five years because manufacturing techniques will not remain static—a cost level of five cents per active element group (equivalent of one transistor and associated passive components) is projected. More complex circuits will be devised and more sophisticated processing techniques utilized.

Automation will be a significant factor here. The conversion of functional requirements first into circuit forms, then proceeding to diffusion mask layouts, will be accomplished by computer techniques rather than by heuristically-based manual approaches. The industry has already learned how to automatically select, position, and interconnect circuit modules in the case of digital systems; only logical statements in Boolean form are necessary as inputs. In many of the processing steps described above, which now involve the manipulative actions of people, automatic machines will take over.

The more complex circuits mentioned above will be the result of a movement to "large-scale integration (LSI)." Here the possibilities will be exploited for interconnecting individual circuits directly on the wafer, negating the need for breaking apart circuit chips and reassembling them electrically in those systems requiring repetitive circuits in great numbers. This requires accounting for those circuits which fail electrical tests and tailoring interconnection patterns so that defective units are bypassed and only operable ones utilized. Such optimal layouts will be obtained automatically by means of computers directly tied into the manufacturing process.

Thus, the trend in the total fabrication process, beginning with a functional requirement and terminating in a validated microelectronic system, is to automation. Computers will develop the layouts, optimize interconnections, and assist in process control. This will reduce labor costs and permit the attainment of powerful logic systems for applications never before feasible on an economic basis. Major advances in manufacturing

technology paralleling phenomenological developments are making this possible.

***Example 9: Thin Wall Iron Castings for Automotive Cylinder Blocks***

Gray iron as a material for automotive cylinder blocks dates back well before 1910 when Ford placed its first order for a production lot of 5,000 cylinder blocks to be cast in gray iron to specified close machining tolerances. Even from this outset of production of automotive cylinder blocks with mean wall thicknesses of approximately .200 inch, it was apparent that engineering requirements could be met with much thinner wall sections. Satisfaction of this, now longstanding, need was delayed for over 50 years and was provided only after the two major technical advances were achieved. These were:

Improved metallurgical consistency, including chill susceptibility.  
More accurate molding equipment.

The building blocks to meet the improved metallurgical consistency included fundamental studies of a mechanism of inoculation to control graphite shape and carbide formation, and research of melting practices as to superheat, slag control, atmosphere control, combustion, and related aspects. No exact date as to the building or assembly of these blocks can be established except that the literature in the 1930's began to show the quest for additional knowledge as to the mechanism for the formation of graphite and the theory underlying the commonplace cupola shaft furnace.

A significant report on inoculation presented before the American Foundrymen's Society by Eash in 1941 summarized the work and theory of many investigators and established some working hypotheses for the gray iron industry.

Other landmark studies included:

Modified ferro-silicons and graphite inoculants to control graphite shape and size by McElwee, Clark, and others (1946-1950)

Influence of alloying, residual chemistry, cupola superheat on structure and machinability of gray irons (1946-1950)

Cupola metal temperature control using hot blast, coke selection, high pressure-structural consistency through controlled blast humidity, control of cupola iron chemistry through control of slag and stack gas, as reported by Lownie and others (1946-1956)

Unlined water cooled cupola as an economical metallurgical reactor as reported by Renshaw, Vennerhom, among others (1947-1951)

This technology developed within the industry and particularly in the Ford Motor Company provided a base for optimism as to the ability of a controlled cupola to supply an iron of significantly improved metallurgical consistency.

Simultaneously with these breakthroughs in the metallurgical area, aggressive process and mechanical equipment development provided shell coring, hot box coring, more precise mold, and coremaking equipment, and closer tolerance assembly techniques.

A significant date was the disclosure in 1945 of the theory of shell molding to produce precision molds and reproducible castings. This so-called Croning process was reportedly used on a small scale in Germany in 1942–1945. After extensive development work over a three-year period (1948–1951) Ford initiated shell molding for crankshafts, camshafts, and valves, and stimulated equipment development to maintain precise molds along with additional new techniques of mold making and mold assembly.

The marriage of the advances in these two technologies by the combined efforts of product and manufacturing engineers sensitive to the potential in this combination, and sophisticated in the many associated disciplines, allowed the gray iron casting industry to provide commercial castings with an average wall thickness of .150 inch. Of equal importance in weight reduction was the reduction in water space between cylinder walls. This made it possible to build engines of greater displacement within given limits of physical size and weight of engine block. The reduction in weight met engineering objectives and provided a material cost reduction at a time when the aluminum industry was making a concerted thrust to make inroads into the cast iron cylinder block market.

#### ***Example 10: Disc Armature Type Motor***

An example of technological advancements in certain industries leading to eventual development of a product in another industry is the Dura-Disc Motor developed by Dura Corporation for automotive applications.

Processing development in the electronics industry, wherein circuitry is produced by metal etching techniques, and the development of suitable permanent magnets by the magnet industry led to the development of a pancake type motor utilizing a permanent magnet field and a printed circuit type disc armature. Motors of this design were made for several years, but with limited usage due to the high production costs involved. The cost of armature production by etched circuit techniques and the high cost of the available Alnico magnets limited application to high-performance servo motors where the response speed of the low inductance armature was required. Specialized performance rather than cost was the dominating factor in such applications.

The advantages of the basic pancake shape, as compared to the conventional cylindrical DC motor with a drum-type armature led to the consideration of applying and developing this type of motor as the driv-

ing means for several potential automotive applications. Electric window operators, and heating and air-conditioning blowers were especially interesting.

Low cost, high-volume production would be required for automotive use. Many technologies were required in this development.

A lower-cost method of producing the disc armature conductor pattern without loss in the total number of torque-producing conductors was the initial objective. This involved basic electrical design and the development of new concepts. Various numbers of magnetic poles were considered in combination with suitable armature-winding circuitry. This resulted in a pattern with a larger number of torque-producing conductors in the complete armature. The armature has multiple layers of conductors in the full winding and utilizes an identical conductor pattern in each of the layers.

Feasibility studies of various methods indicated a mechanically stamped conductor pattern would be the most economical. The ability of precision die makers to produce dies with suitable tolerances for stamping the laminations was a governing factor. Improvements in die technology in the areas of punch construction and dynamic characteristics were vital to the success of the die development.

A typical armature consists of four layers of stamped conductors. Two laminations are assembled back-to-back with an annular layer of insulating and bonding material sandwiched between the laminations. This assembly is then bonded to maintain conductor location. An inside diameter shearing operation then separates the conductors forming inwardly extending matching tabs. These tabs are welded together. Identical pairs are assembled with an annular layer of insulating and bonding material sandwiched between the pairs. Bonding of this assembly is followed by an outside diameter shear operation to form outwardly extending tabs. These are welded together to complete the armature winding. Hubs and output shaft are also bonded to the assembly to complete the armature.

Techniques developed at this point allowed the use of the newer, high-coercive, low-cost, ferrite magnets. The total combination pointed to a practical, reliable, low-cost motor. This then became the basis for development of manufacturing and processing technology for low-cost, high-volume production of motors.

The combined talents of research, development, processing, and tooling engineers were utilized to finalize the design and establish production techniques. Major manufacturing technologies involved were the development and application of suitable insulating and bonding materials; processing methods to provide a rigid armature capable of satisfactory operation in the environmental conditions; and the selection and development of economical methods for making the multiple electrical connections between the conductors. These methods were initially evaluated

utilizing laboratory type equipment followed by procurement and set-up of pilot plant equipment and fabrication of armatures to establish production feasibility and expected production costs.

The advantages of this basic motor shape in automotive applications have been recognized. In the case of the heater blower, the disc motor virtually becomes a part of the blower housing with very little projection. Projection of a conventional motor beyond the housing often complicates heater installation and has, in some instances, required that heater blower motors actually project through the fire wall. In electrically operated power windows the package size and shape allow for thinner door sections and provides the stylists with more freedom in door design.

This Dura Disc Motor development has resulted in the creation of a new and useful product for integration into present and advanced concepts of automotive design. The inherent advantages of this motor will provide for improved performance, greater reliability, and lower costs.

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1. For an excellent description of design optimization see "The Deacon's Masterpiece, or the Wonderful One-Hoss Shay," by Oliver Wendell Holmes.

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## CASES OF RESEARCH AND DEVELOPMENT IN A DIVERSIFIED COMPANY

by C. GUY SUITS and ARTHUR M. BUECHE

### Introduction

Because of its magnitude, the problem of utilizing the Nation's research capabilities to achieve national goals has no real precedent or analogue. However, on a somewhat smaller but by no means miniature scale, the research experience of a highly diversified industrial firm offers many analogues in the application of scientific research to the solution of practical problems that may be helpful to those concerned with the development of policy and practice in applied research at the national level.

We shall talk about research and development in the General Electric Company not because of chauvinism, but because we know it best. The firm is, of course, an industrial organization of substantial size. It is possibly the most diversified manufacturing company in the world. Its products range from nuclear reactors to night-lights, from jet engines to electric toothbrushes, from space vehicles to transistor radios, from computers to toasters, from telescope mirrors to glue, from weapons systems to cutting tools, from X-ray equipment to clocks; it has approximately 700 product lines. It is geographically and managerially decentralized. It has been engaged in scientific research for more than two thirds of a century, and its research—like the business itself—is extremely diversified, covering virtually all areas of the physical sciences and extending into the life sciences.

Consideration of research and development in a corporation should begin with a discussion of how the size of the activity is to be determined, what its objectives are to be, how the areas of science and technology are to be chosen, and how the money, people, and facilities are to be allocated to the chosen areas. These features are exceedingly important, and they have been widely discussed. They are also the subject of other contributions to the work of this Committee; consequently we shall not be concerned with them in this discussion. It will be assumed that the objectives, the organization, and the broad research areas have been decided, and we shall consider the actual conduct of the work: how particular projects originate, the factors that determine success or failure, when to continue or when to stop the work, how the transition is made from re-



search to production. These features are of the greatest importance for successful corporate research and development.

In a diversified corporation the problems of managing the methodology of research and development—its sources, its tactics, its uses—as well as of establishing its policy and rationale are exceedingly complex. They pose a quandary not encountered in a corporation that is more homogeneous and has a single center of business planning and a single pace of technological change. Within a diversified corporation the individual businesses generally need laboratories of their own, laboratories that specialize in those areas of science and technology that are important for only the particular businesses. On the other hand, the central corporate laboratory works in those areas that are common to several components, are more speculative and longer range, or will benefit from the economies of scale possible in a corporate laboratory.

The purpose of this essay is to illustrate some of the features of research and development in the central, corporate laboratory of the large, diversified company with which the authors are most familiar.

Within this laboratory there have developed over the years literally hundreds of examples of industrial research projects—some highly successful, some completely unsuccessful, some still in question—which include broad spectra of activities: the simple satisfaction of scientific curiosity, discovery, applied research, development, an impact in the marketplace or on the production line.

Even the most cursory glance at a few of these stories makes it clear that the basic lesson to be learned from a study of them is that *in this diverse environment no two situations in applied research are ever quite alike.*

First, there is great variety in the types of innovations involved: new or improved products, new manufacturing processes, new applications of old products. Also, the “nucleating event,” which sets off a chain of applied research, can come from many sources: an unfolding opportunity in the world of fundamental scientific research, the posing of a particular design or production problem, the delineation of a marketplace need, a specific research-management decision—any or several of these may help trigger the creative powers of a scientist. The time element in applying research to practical purposes ranges all the way from examples of ideas that were hopelessly premature to situations of great urgency. And, finally, innumerable factors are involved in making the transition from research discovery to useful application: the continuing initiative of the individual researcher, the interaction of research management and product management, the demonstration of technical feasibility, the point at which economic analysis is undertaken, personnel transfers, pilot-plant construction, and a host of others.

To illustrate these features of industrial research and development, we will give a number of case histories and then return to a discussion of the elements of methodology that they illustrate. The examples selected are not necessarily the most successful stories, nor are they concerned with the largest technical programs in the company's history. Mainly, the examples are taken from those that nucleated in the company's central research organization. Massive efforts, such as the development of the boiling-water reactor or jet engines or computers, for example, are not included because in such large programs an increasing percentage of the work is "development" rather than the "applied research" that is the subject of this study. The largest programs generally have involved the creation of major technical organizations for that purpose alone.

As noted previously, we do not presume that all the lessons that have been learned from these examples over the years necessarily will be directly applicable to the Federal Government's massive problems of managing applied research and technology transfer on an unprecedented scale. However, the variety inherent in the cases chosen for summary here should provide insights into the handling of the even more varied situations occurring in research and development at the national level.

### Case Histories

#### *Man-Made $\otimes$ Diamond*

The history of attempts to synthesize diamond probably started in 1797, when diamond was first known to be a form of carbon. Converting an abundant and inexpensive substance—carbon—to a comparatively precious material—diamond—has proved to be a tantalizing temptation to scientists and technologists over many decades. But the conceptually simple task of "rearranging" the atoms of graphite into the more compact atomic configuration that is diamond had proved insurmountable until it was accomplished by General Electric (GE) scientists late in 1954.

Beginning in 1941, GE took a more than casual interest in diamond synthesis. GE, along with the Carborundum Company and the Norton Company, joined in financial support of a five-year investigation conducted by the late Percy Bridgman of Harvard. However, because of the war, only a two-year portion of the five-year program was completed and no success was achieved. After the war, the Norton Company became interested in continuing the work, acquired equipment from Bridgman, and undertook a program. Late in 1950, overtures were made by Norton to the management of the GE Research Laboratory, proposing a joint project; however, no agreement was reached between the parties.

This new flurry of activity triggered a fresh look at GE's stake in the field by Research Laboratory management.

There were compelling reasons why the successful synthesis of diamond would be of substantial relevance to GE. The company's Metallurgical Products Department (formerly the Carboloy Department), based in Detroit, is one of the Nation's largest consumers of industrial diamonds. Diamond is necessary for the cutting of cemented-carbide materials, used in turn as the cutting tools of lathes, milling machines, drills, and other machine tools.

From the standpoint of the country's national security, it was foreseen that an assured domestic source of industrial diamond, so vital for key defense industries, might make the United States completely independent of industrial diamonds from the Congo.

This fresh appraisal of GE's stake in synthetic diamond production had stirred great enthusiasm among several young and talented members of the Research Laboratory staff. Their enthusiasm was undampened by the long history of failures, and they were anxious to "have a go" at this age-old challenge by taking some fresh new approaches.

A position paper from the Research Laboratory to the GE Engineering Council, then responsible for reviewing major engineering development programs, stated (in part):

Although the production of the synthetic diamond would be a primary objective in an investigation of this kind, the whole area of pressure and temperature is largely unexplored because of the great difficulty of doing such experiments. Therefore, it would be a fruitful region for exploring the properties of other materials whose structures are favored by high temperature and pressure.

The present industrial diamond business in this country amounts to approximately \$35 million annually at present prices (\$7000 per pound). For hardness no other known material approaches diamond, which is an absolutely unique substance in this respect. If it could be produced more cheaply, it would undoubtedly have a very important effect on all types of manufacturing operations.

The question facing the Research Laboratory is whether the prospective gains, both in the generation of a new business, and in the impact of the new product on our manufacturing operations, justify the considerable research expenditures. The Research Laboratory may wish to undertake work in the field of high pressure-high temperature synthesis with more general objectives than provided by synthetic diamond as a goal. The investigation in this case would be of a more modest character, however, and is not before the Engineering Council for discussion.

The specific objective of synthetic diamonds as a portion of a high-pressure research program was presented to a meeting of the GE Engineering Council held in Schenectady, N.Y., on March 23, 1951. On the basis of the very substantial enthusiasm and encouragement there expressed, the management of the Research Laboratory decided to proceed with the diamond investigation.

Exploratory work began in several directions by the initial Research Laboratory Team of A. J. Nerad, F. P. Bundy, and H. M. Strong. Three other scientists, H. T. Hall, R. H. Wintorf, Jr., and H. P. Bovenkerk, were added to the project during 1951.

One thrust would be in the field of "superpressure"—combining extremely high pressures with high temperatures. Other alternatives were

explored in parallel, however, including work on crystal nucleation theory and experiment, in the hope that diamond crystals might be grown at low pressures. They never were.

Studies of transition data for graphite-to-diamond were the initial steps. Pressures were speculated to range from 30,000 to 100,000 atmospheres (450,000 to 1,500,000 lb per sq in), and temperatures above 700° would be required. The speculation could not be precise, and these estimates were quite approximate. "How long these temperatures and pressures would have to be sustained was hardly a matter of speculation; it was a complete mystery—one of many, many mysteries," an observer has since remarked.

Work proceeded on the design of pressure chambers, using at first an ancient 400-ton press; later a new 1,000-ton unit was installed. Development work continued, using the strongest available materials, unique prestressing techniques, and geometrically complex designs, all devised by the research team. Further work, using molten-metal catalysts, proved profitable, and the first truly identifiable diamonds were produced—and repeatedly reproduced—in 1954, less than four years after the beginning of the project.

The achievement was not as simple and clear-cut as this brief description might suggest. For example, there was a substantial "process" problem in addition to the need for apparatus development. One participant in the project has said, "As we later learned, much of the apparatus-development work might have been unnecessary if we had only known the process." As with all such projects, a variety of "blind alleys" was explored, including sources of carbon, direct conversion, electrolysis experiments, and others.

Refinements continued in the technique of producing Man-Made diamond, as the new material was named, the experimenters rapidly increased the yield of diamonds, achieving 30 to 40 mg per run with the best systems during March 1955.

In April 1955, a manager of the diamond project was assigned by the general manager of GE's Metallurgical Products Department. His original staff consisted of himself, a secretary, and another engineer. In about two months' time, after buying a small press and borrowing some equipment from the Research Laboratory, diamonds were produced in Detroit on a laboratory scale in a small press. In the meantime, large, high-capacity presses had been ordered, and there was a constant and continuing interchange of men and information between Detroit and Schenectady. (Two of the Research Laboratory men—an engineer and a technician—who had been actively engaged in the superpressure studies transferred to the staff of the diamond project in Detroit about two to three years after it got under way.)

As progress continued in the Research Laboratory, an intensive effort was made to produce enough diamonds for testing on a grinding wheel. It took 100 runs, around the clock, to make the 24 carats required for tests.

On May 18th these tiny diamonds, which showed little visual resemblance to mined diamond grit, were shipped to Detroit and subjected to a variety of abrasive tests. Later, additional diamonds were made and tested in abrasive wheels. The test results, awaited with considerable anxiety by the research group, were surprisingly satisfactory in one category of diamond usage, although still grossly inadequate in others. Nevertheless, this added to the scientific triumph of producing diamonds the very good prospect of their commercial utility, given adequate product and process refinement and development.

The researchers and the Detroit engineer, aided by scientists and engineers in the General Engineering Laboratory of the company, then launched the crucial "transition and engineering phase" of the project which was to consume two full years and even extend beyond the first commercial introduction of GE Man-Made diamond. The numerous problems and questions they confronted included gaining an understanding of why these diamonds performed differently from mined diamonds, learning how to improve the apparatus and process capabilities, and becoming familiar with technological and product needs of the existing diamond industry.

The full nature of the opportunities presented by this scientific breakthrough became better understood. The diamond abrasive industry needed not only another source of supply, but also a source of technological understanding of how diamonds performed, how that performance could be improved, and where new applications could be found. The business strategy that emerged resulted in the first product differentiation in this segment of the diamond industry and greatly improved abrasive tool performance; also it stimulated increased commitment by others in the industry to development of improved application technology. Today's diamond grinding wheels and cutting saws will perform five to ten times more effectively than the typical wheels and saws of the 1957 period.

The elapsed time from the beginning of the research project to commercial production was less than six years despite the magnitude and multiplicity of the problems encountered. In the words of one Research Laboratory official, it represented "something of a world's record in bringing a major new scientific discovery from the test tube to the marketplace."

The "diamond mine" at Detroit is now one of the largest sources of industrial diamond in the world, and American diamonds, nonexistent only one decade ago, are now used in every important industrial nation in the Free World.

A substantial portion of the proceeds of this business is being reinvested in further high-pressure research, development of larger and better Man-Made diamond crystals, and studies of diamond application technology to further the understanding of abrasive systems.

As an additional result of the success of research which led to diamond synthesis, superpressure work has become a major project at laboratories around the world. From further explorations into this new branch of science, GE researchers have created more than 30 new forms of matter. Others are being investigated.

### Key Personnel

- A. J. Nerad*: BS (mechanical engineering), University of Wisconsin. GE, 1923. Retired.
- F. P. Bundy*: BS, Otterbein College; MS and PhD (physics), Ohio State University; Research Associate, Harvard University. GE, 1946. Presently physicist (general physics, optics, bearings, lubrication, friction, wear, acoustics, vibration, superpressure, thermal insulation), GE Research and Development Center.
- H. M. Strong*: BS (chemistry, physics), Toledo University; Pennsylvania State University; Ph D (physics), Ohio State University. GE, 1946. Presently physicist (general physics, optics, superpressure, thermal insulation), GE Research and Development Center.
- H. T. Hall*: BS (chemistry), University of Utah; Bowdoin; Massachusetts Institute of Technology; Purdue University; PhD (chemistry), University of Utah. GE, 1948–1955. Presently Director of Research, Brigham Young University.
- R. H. Wentorf, Jr.*: BS and Ph D (chemical engineering), University of Wisconsin. GE, 1951. Presently physical chemist (general physical chemistry, critical phenomena, thermochemistry, geochemistry, friction, superpressure), GE Research and Development Center. Currently on one-year research leave.
- H. P. Bovenkerk*: University of Chicago; BS, University of Michigan. GE, 1947. Presently Manager, High Pressure Research, Diamond Business Section, GE Metallurgical Products Department, Detroit, Michigan.

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### **Tunnel Diode**

During March 1958, R. N. Hall of the Research Laboratory read an item in the *Physical Review's* "Letters to the Editor" (January 15 issue), in which Japanese physicist L. Esaki described the "tunneling" effect of semiconductors. The item aroused Hall's interest, but he put it aside because of the rush of current and on-going projects.

J. J. Tiemann, a young Ph. D. from Stanford, had just been hired by the laboratory, and Hall began looking for a project to catch Tiemann's interest. Tiemann was recognized as a good physicist who was especially interested in electronic applications. Hall remembered reading Esaki's paper and surmised that if his interpretation was correct, tunneling could be an important phenomenon. Indeed, Tiemann became very enthusiastic.

Hall and Tiemann worked together and Tiemann soon devised some diodes that showed the tunneling effect. Soon after he had oscillators operating in the hundred-megacycle range, and he found ways to make devices small enough so that they were promising as electronic components.

From the very first days of this project, the Advanced Semiconductor Laboratory of GE's Semiconductor Products Department, Syracuse, N.Y., had been kept abreast of developments. The liaison activity intensified

late in 1958. From the beginning, it was seen that these new devices had promise. It was a brand-new phenomenon and seemed to have everything desirable for high-speed switching and amplification. The group in Schenectady, along with N. Holonyak and I. A. Lesk in Syracuse, worked together in developing the tunnel diode.

One of the things recognized immediately was that the device required entirely new concepts in electronic circuits to take full advantage of its capabilities; it was not just a direct replacement for a transistor: Tiemann was very active in developing new circuits and applications. There also was a serious effort in Syracuse, in the electronics application group there, to develop circuits and find a wide variety of applications in order to build a market for tunnel diodes.

Transition to commercial use was rapid; the Semiconductor Products Department had the devices on the market in pilot-line quantities within two years after discovery, and in full production within the next year. They achieved this success even though tunnel diodes require extremely critical and precise manufacturing techniques. They must be etched to very small dimensions, about a micron in diameter at the junction. The resulting structure is extremely fragile, but this approach is necessary to get the best possible performance, especially for high-frequency applications.

Within a relatively short time, tunnel diodes became a "standard" semiconductor product. They have found their way into computers, television sets, communication equipment, nuclear controls, satellites and space vehicles. These devices have oscillation frequencies higher than those of transistors, making them suitable for a wide range of communications applications. Because the tunnel diode is at least 100 times faster than transistors, and can be made to use only about 1/100 as much power, it has wide use in computers. Moreover, the diode is insensitive to temperature changes, in contrast to the transistor. Silicon tunnel diodes work at temperatures as high as 650°F; conventional silicon diodes will not operate above 400°F. Also, the device resists the damaging effect of nuclear radiation, outranking transistors by more than 1000 to 1.

As Hall has pointed out:

When something new turns up, you must make a decision—depending on your manpower and other resources—whether to pursue it (and with what intensity), put it aside until resources are available, or forget about it. Fortunately, Tiemann was available to start work on the tunneling concept as soon as he came with us.

### Key Personnel

*R. N. Hall:* BS (physics) and PhD (nuclear physics), California Institute of Technology. GE, 1942. Presently physicist (junction physics, crystal growth, recombination kinetics, junction lasers), GE Research and Development Center.



- J. J. Tiemann*: BS (physics), Massachusetts Institute of Technology; PhD (physics), Stanford University; Research Assistant, Stanford University. GE, 1957. Presently physicist (junction physics, circuit theory), GE Research and Development Center.
- L. Esaki*: Diploma (nuclear physics), University of Tokyo; PhD (solid state physics), University of Tokyo. Presently heads group studying semi-metals and junction properties of semiconductors, IBM, Thomas J. Watson Research Center, Yorktown Heights, New York.
- N. Holonyak*: BS, MS, and PhD (electrical engineering), University of Illinois. GE, 1957–1965. Presently Professor of Electrical Engineering, University of Illinois.
- I. A. Lesk*: BSc, Alberta; PhD (electrical engineering), University of Illinois. GE, 1951–1961.

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### P-Zero Insulation

Late in 1947 a young engineer from GE's Refrigerator Department, then located in Erie, Pennsylvania suggested to the director of research that there was a real need for a more efficient and compact thermal insulation for household refrigerators. He pointed out that, of the total volume enclosed by the refrigerator cabinet, more than half the space was occupied by insulation. Discovery of a more efficient insulation would give the housewife more space *inside* the refrigerator without increasing the *exterior* dimensions, and possibly this would be attractive economically—for no increase in structure would be required. The logic was unassailable, and everyone could immediately see the competitive advantage to be gained in the marketplace from such a "breakthrough."

The research director, recollecting fundamental work on heat transfer in evacuated powders done 30 years earlier by I. Langmuir, asked H. M. Strong and F. P. Bundy to explore the problem. He reminded them that A. W. Hull of the Research Laboratory had also done some work in the mid-1940's on evacuated wall panels.

Strong and Bundy quickly came to the conclusion that to achieve a thermal insulation significantly better than existing insulation, they must use evacuated panels. But such panels presented a basic problem: the

crushing force of the atmosphere tended to push together the two sides of the panel. Some kind of mechanical bridgework was therefore necessary to keep panels separate. But the bridgework provided a "thermal path" for heat loss, and thus canceled some of the efficiency of the vacuum.

The two scientists knew that some type of nonconducting bridgework was required, so they experimented with powdered glass and alumina. Efficiencies achieved were favorable, but short of the design goal.

Then Strong hit on the idea of thin rods of glass fibers stacked up like a pile of jackstraws. In fact, Bundy has since "accused" Strong of playing jackstraws the night before the discovery. The stacked glass fibers were strong enough to support the walls when the panels were evacuated.

Experimental panels using the new technique were built and tested. "The initial results were so good we didn't believe our data," Bundy has said. The results were two orders of magnitude better than anything else available, so the experiments were repeated two or three times for verification.

Flat panels the size of refrigerator doors were made and tested at the Refrigerator Department. Initially, there was concern that the panels would not hold their vacuum for 20 years—the design life of a refrigerator. The question was resolved satisfactorily after some very careful work on diffusion rates. Bundy has some of the early prototype panels in his home freezer, and they have maintained their integrity for 17 years.

At the Refrigerator Department, further work continued on mass manufacturing techniques, particularly in the troublesome areas of hinge design and thermal breakers. Cost analysis and market studies were launched. Appearance design factors were considered.

Then a series of events took place that put the thin-wall "P-Zero" insulation into limbo.

The Refrigerator Department was moving to the new GE Appliance Park in Louisville, Kentucky. During the same period, a new compressor design was demanding top engineering attention. Management decided not to attempt *two* major design changes simultaneously. Appearance design presented a major problem because refrigerators of the early 1950's still had curved, bulbous surfaces that presented staggering manufacturing problems for the new insulation. (It was not until the late 1950's and early 1960's that the new "built-in" and "squared off" look took over in household appliance design.) Also, market surveys began to show that the housewife wasn't too enthusiastic about smaller-appearing appliances. Market research also pointed out that the cost-per-cubic-foot of home construction is actually quite low so that the customer might find it less expensive to pay for the extra space needed by a conventional refrigerator than to pay the extra cost of the smaller refrigerator with "P-Zero" walls.

The thin-wall insulation project for refrigerators dropped from sight in the early 1950's, and lay dormant for nearly a decade until a new GE

group known as Patent and Technology Marketing began trying to sell rights to the idea to non-GE organizations. Suggested users were companies transporting liquid air, oxygen, nitrogen, or natural gas.

The patent for "P-Zero" was issued in 1964 and a license has been given to an English company that has considered use of the concept in refrigerated ships for carrying liquid methane from North Africa to England. Similar applications have been suggested for storage tanks and transfer lines, as well as refrigerated trucks and railroad cars, such as those used for hauling frozen foods.

But "P-Zero" insulation never achieved the originally intended use in refrigerators, and almost 20 years after the original invention, one of the "world's best thermal insulations" still has not been translated into a commercial application.

### Key Personnel

*F. P. Bundy:* BS, Otterbein College; MS and PhD (physics), Ohio State University; Research Associate, Harvard University. GE, 1946. Presently physicist (general physics, optics, bearings, lubrication, friction, wear, acoustics, vibration, superpressure, thermal insulation), GE Research and Development Center.

*H. M. Strong:* BS (chemistry, physics), Toledo University; Pennsylvania State University; PhD (physics), Ohio State University. GE, 1946. Presently physicist (general physics, optics, superpressure, thermal insulation), GE Research and Development Center.

*A. W. Hull:* BA and PhD (physics), Yale University; Faculty, Worcester Polytechnic Institute. GE, 1914. (Deceased, January 22, 1966.)

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### Vacuum Circuit Breaker

Production of equipment for the electric utility industry is an important and continuing aspect of GE's business. Thus basic and applied research on challenges in the field is (and has been) carried on by laboratories in various product departments as well as by the central research laboratory.

Information interchange between product departments and the centralized Research and Development Center is continuous—by personal correspondence, published articles, seminars and conferences, and personal discussions. This is a two-way flow: the problems of the product departments come to the attention of Research and Development Center personnel, and the ideas, concepts, and results of the scientists are transmitted to the product departments.

One of the key pieces of equipment in any electric system is a circuit breaker, a device that will interrupt and break large electric currents and thus control the flow of energy and provide the protective function for the system and its equipment. A circuit breaker performs in large scale the functions of a switch, or sometimes an overload switch in a typical home wiring system. Over the years, high-power circuit interrupting technology has been developed around air-blast, oil-blast, and magnetic blow-out methods of arc suppression.

Ever since the early 1920's, engineers have been aware of the advantage of interrupting large currents in high-voltage circuits by separating two metal contacts enclosed in a vacuum. Because the high breakdown strength of high vacuum makes possible a gap less than one-half inch in length in typical applications, the actuating mechanism for the contact stroke can be relatively simple and fast acting. Also, the arcing time may be limited to one-half cycle, and energy thus dissipated in the vacuum switch is comparatively small. The fast recovery of the vacuum gap eliminates the necessity for an interrupting medium, such as oil or gas, with its attendant problems of large structure and mechanical maintenance. With a successful completely sealed switch, the very serious fire and explosion hazard is eliminated.

It was long recognized that these factors all indicated the *possibility* of a simple, compact, high-speed device for current interruption and offered a real incentive for the development of a practical vacuum switch. But it took nearly 40 years before the concept could be transformed into a practical and reliable device for electric utilities.

The vacuum switch was invented and actually demonstrated in the early 1920's by Professors R. A. Millikan and R. W. Sorensen at the California Institute of Technology. Shortly afterwards, GE acquired patent rights to the vacuum switch from CalTech and began an intensive development program. While some of the early experiments in 1930 showed great promise, it soon became evident that the conceptually attractive idea of a vacuum switch embodied some very serious problems, and that the most basic of these difficulties was the maintenance of vacuum in a sealed-off device.

In these early demonstration experiments the vacuum was continually maintained by a pump; repeated attempts to build a sealed-off device—at the Research Laboratory and elsewhere—failed. Analysis showed that the vacuum problem comprised several problems. First and foremost was the achievement of an initial high vacuum at least two orders of magnitude better than industrial practice in lumps and electron tubes at that time. Second, the evaporation of contact metal during an interruption invariably released gas which spoiled the initial vacuum and thus ruined the switch.

There were secondary problems, too, such as the tendency of metal contacts to "cold weld" together in a high vacuum, and the significant problem of the arc interrupting before the current zero—leading to serious voltage surges in the system. If these problems had been solvable, there would have emerged the fundamental problem of building a large, strong vacuum device. Basically, however, the early attempts were doomed to failure because of the lack of supporting technology in vacuum practice, and in metallurgical processing.

During the years following the initial 1930 work, repeated studies were made on various problems associated with the vacuum switch. However, the vacuum switch program that finally achieved success was started in the Research Laboratory in 1952 by J. D. Cobine and a number of his associates. The background for the decision to start a new program included the fact that, in the intervening period, rapid advances had been made in vacuum and metallurgical processing and technology. New electronic instrumentation made it possible to study vacuum-arc phenomena. Basic scientific knowledge acquired from research on metal vapor arcs could reasonably be expected to benefit vacuum switch design.

The production and maintenance of a high vacuum in the interrupter, regardless of the severity of switching service, continued to be key problems. Fortunately, by this time A. W. Hull had developed Fernico for use in making vacuum tubes. Fernico was an alloy which, thanks to its carefully selected expansion characteristics, permitted the design of large glass-to-metal seals capable of withstanding the mechanical and thermal stresses expected in high-power vacuum interrupters. Furthermore, vacuum technology—developed in connection with surface-chemistry research—had advanced to the point where vacua of the order of  $10^{-9}$  mm Hg could be produced and measured routinely. Thus, progress over the years had solved two of the basic problems.

However, the release of gas from contact materials (other than molybdenum and tungsten) during arcing was the key problem that had not been solved. The discovery of metals with the required freedom from gaseous impurities evolved from a suggestion of M. H. Hebb when electrodes were made from single-crystal copper and especially prepared zone-refined copper. (Zone refining was a technique developed first at Bell Telephone Laboratories to produce extremely pure silicon and germanium for semi-conductor devices such as transistors.) Copper crystals drawn from the melt by W. W. Tyler were remarkably free of gas, and gave the first indication of a major breakthrough.

The first zone-refined copper was especially prepared for vacuum-switch contacts by F. H. Horn, and arcing tests revealed the surprising fact that the gas content was less than one part in 10 million.

This work represented a major achievement in vacuum switch contact technology. It probably could have only come about by the concurrent

rapid developments in the semiconductor field and the close cooperation of scientists with many interests. The discovery of a process for preparing copper with the desired degree of purity now made it possible, with modifications, to prepare a large number of gas-free electrodes of other metals, alloys, and compounds so that they could be examined for other desirable characteristics required of high-power vacuum interrupters. Much of this work was carried on by J. M. Lafferty and his associates.

While fundamental research is very valuable in pointing the way toward a new commercial product, it does not bring it to fruition. It takes an aggressive manufacturing organization with judgment, imagination, and a willingness to take a chance, to incorporate quickly new scientific knowledge into the development, engineering, and manufacture of new products.

In 1955, the GE Switchgear and Control Division started a development program to investigate the technical feasibility of vacuum interrupters for switchgear applications. This required further investigation of many related problems, such as electrode and shield design, continuous current-carrying ability, high potential and impulse strength, contact life, and materials and processing. All these problems were solved, and a promising design of a heavy-duty vacuum circuit breaker was built, tested, manufactured, and successfully marketed.

The Switchgear and Control Division had a nucleus of capable people to initiate this development program; otherwise the transition to commercial application would have been impossible. However, the stimulus of their work and the knowledge and experience acquired in carrying it forward have in turn helped create a first-class group with expertise in arc interruption and transient electric phenomena. This group has become a significant technical resource for the entire company and recently has been designated a "Center of Research" for the company in these subjects. It was one of the first "Centers" so designated in a program conducted by the centralized research and development organization to afford recognition to outstanding technical groups in company operations and call attention to their special skills so greater use could be made of them by other components of a large and highly decentralized company. More than 20 such "Centers of Research" have now been designated, and in several of them work is supported by central corporate funds, insuring and encouraging their performance of research and development of broad general interest to the company.

Research and related facilities connected with the vacuum circuit breaker project required an investment of more than \$5 million. It is now clear that the old dream of the high-power vacuum switch has become a reality, and that it will become the dominant circuit interrupting technology of the future.

### Key Personnel

- J. D. Cobine*: BS (electrical engineering), University of Wisconsin; MS (electrical engineering) and PhD (electrical engineering and physics), California Institute of Technology; Faculty, Harvard University; Faculty, California Institute of Technology. GE, 1945. Presently physicist (discharge in gases, arc phenomena), GE Research and Development Center.
- A. W. Hull*: BA and PhD (physics), Yale University; Faculty, Worcester Polytechnic Institute. GE, 1914. (Deceased, January 22, 1966.)
- W. W. Tyler*: BA and PhD (physics), Cornell University. GE, 1950–1963. Presently Director of Research, Xerox Corporation.
- F. H. Horn*: BA (chemistry), Oberlin College; PhD (chemistry), The Johns Hopkins University. GE, 1946. Presently Manager, Semiconductor Crystal Unit, GE Research and Development Center.
- J. M. Lafferty*: BS, Western Michigan; MS and PhD (electrical engineering), University of Michigan; Research Physicist, Carnegie Institute of Washington. GE, 1942. Presently Manager, Plasma and Vacuum Physics Branch, GE Research and Development Center.
- M. H. Hebb*: BA, University of British Columbia; University of Wisconsin; PhD (physics), Harvard University; University of Utrecht; Instructor, Duke University. GE, 1949. Presently Manager, General Physics Laboratory, GE Research and Development Center.

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### Etched Particle Tracks

As fission fragments—produced when an atom of a heavy element, such as uranium, undergoes fission, splitting into two fragments—pass through solid material, they create tiny trails of damage, a few atoms in diameter. On November 8, 1961, R. M. Walker and P. B. Price, assisted by Mrs. E. Fontanella, discovered that in the insulator, mica, these radiation tracks could be etched-out to form uniform-sized holes by dipping the material into a suitable reagent. The holes were so tiny that, like the tracks, they had to be observed with an electron microscope.

In the hands of Walker and Price, who were joined soon thereafter by R. L. Fleischer, also of the GE Research Laboratory, this simple discovery has led to an astonishing variety of scientific and commercial results—from a method of geological dating to a new biological filter, from testimony about the age of fossil primates to a technique of measuring nuclear radiation, from evidence about the solidification of celestial matter to a new scheme of neutron radiography. And the list does not end with these.

The question may be asked: How could the research laboratory of a large company justify the original goal of the research—to find and study "fossil" cosmic ray tracks in meteorites? Only the most general arguments could link GE's aerospace activities to an interest in the radiation history of lunar or meteoritic materials. There was also a general but somewhat tenuous connection between the company's nuclear power business and an interest in fission tracks. Moreover, no company business faced a technical problem that the research, as originally conceived, was explicitly designed to solve; in short, the research did not arise from any direct or specific need of GE's businesses and was related to them only in a general way. Why, then, was the research condoned, supported, and encouraged in an industrial laboratory?

The answer is that a large company and a large laboratory can invest a small fraction of its funds in speculative ventures in research; these ven-



tures promise, however tentatively, departures into entirely new businesses. Although existing needs and problems dictate the largest part of research in industry—or in the Nation, for that matter—experience has shown repeatedly that studies begun solely to grapple with fundamental questions of science can be rewarding to industrial technology, providing advances for which there was no recognized pre-existent need, however strong that “need” might become afterwards.

The research on particle tracks, conceived in pure science, was from its inception nevertheless sifted for technological advances. When its discoveries could be embodied in inventions, this was done and applications for patents were submitted. But in the beginning, no one could be certain that the inventions, though technically sound, would have any commercial value. At that point a patent application by its implied protection served primarily to permit the research to go forward in the healthiest and most productive scientific tradition, being reported through papers and talks to the scientific community, without unduly jeopardizing the commercial value that the results might subsequently be found to have.

By June 1962, another step had been taken toward recognizing and evaluating the worth-while applications of this research: a study of potential applications had been made by three scientists other than the inventors themselves, scientists who had had greater experience in technology and applied research though almost none in the subject of the research itself. This study, which attempted to identify the potential applications and to assess their importance, was made at a time when the etching technique had been successfully applied only to mica and to a few other crystalline minerals, and when observations were still being made only with the electron microscope. The study devoted a good deal of attention to the possible uses of sieves made by the particle-track technique, but other than remarking that the cost per *hole* would be considerably less in mica sieves than in other sieves then on the market, the study was able to do little more than list a variety of potential applications on which additional work was needed. Among these was “blood analysis.”

Five months later, the “blood analysis” item on the list became of primary importance.

One day in November 1962, Price received a phone call from Dr. S. H. Seal of the Memorial Sloan-Kettering Cancer Center in New York City. Seal was attempting to isolate and detect cancer cells in blood by filtering the blood through sieves that would hold back the larger, more rigid cancer cells while allowing others to pass through. Other filters he had tried were unsatisfactory and he was searching for more suitable types. By word of mouth he had heard of the sieves that could be made with fission fragments. Price agreed to etch holes 3 to 4 microns in size in 1-inch-diameter pieces of mica. But when Seal tried them they broke, and he asked for more.

Further work continued on the sieve project, with cooperation from Dr. H. M. Rozendaal, a physician at the laboratory. He began to consult and guide the biological aspects of this work and also became the natural intermediary with Seal. Rozendaal could speak the language of both the research scientists and the MD, a skill that undoubtedly prevented a mix-up in communications, and gave both parties added confidence in each other.

Fleischer also continued to search for tracks in other materials. In February 1963, he made a discovery that was to have widespread ramifications: fission fragments caused etchable tracks in GE plastic, Lexan® polycarbonate resin. He also discovered other plastics which showed the tracks. Plastics had several advantages; one of the most important was the fact that they could be handled and used more easily than large sheets of glass (which was also found to give etchable tracks) or mica. Early in May, filters were on their way to Seal, and his experiments were promising. The cancer-bearing blood flowed through the filters leaving the cancer cells deposited on the transparent plastic and allowing them to be strained *in situ* and examined under a microscope.

But Seal needed many more filters for clinical tests. Existing facilities at the laboratory were inadequate, and to construct adequate facilities required expenditures and manpower beyond those needed for the fundamental studies. Thus, for the first time the people concerned with this research program were confronted with decisions that were beyond the demands of research alone. Should GE attempt to supply the filters for Seal as a scientific courtesy or as a contribution to worthy medical research? It would be going beyond the normal bounds of scientific courtesy to spend substantial amounts of money to serve the research of another scientist, however, admirable that research might be. Yet the emotional and humanitarian overtones of cancer research placed this question in a unique position. These potentially thorny questions were resolved by a simple fact: if Seal's research were successful, there would be a reasonable chance to create a new business and the necessary expenditures could be justified by this fact alone.

The decision was made to scale-up production of plastic filters so as to be able to supply Seal with enough of them for clinical tests and to take the next step toward commercial development should all go well. By the end of August 1963 a pilot plant was under way; by October it was in full operation and Seal was receiving an adequate supply of filters. His clinical work culminated in a paper published in May 1964 in the *Journal Cancer*; in it he reported encouraging results from tests of the blood of 50 patients known to have cancer.

Seal's technique is under study by an NIH-sponsored medical panel which has the task of developing a foolproof method of detecting cancer

in the early stages. The use of this plastic filter to identify cancer cells has recently been extended to body fluids other than blood. Dr. Eileen King, a University of California cytologist, who directed this work, is now investigating, under NIH sponsorship, the use of the filter for the diagnosis of cancer of the uterus and cervix using the new innovation of the self-administered irrigation method. Another application of potential importance in national defense is the use of the filter in tape form as the reference membrane in the U.S. Army's biological warfare agent detection program.

Further economic and marketing studies showed that the filters could become a commercial product, manufactured and sold profitably even though the anticipated sales were small by GE standards. By this time, the company's Nuclear Energy Division had prepared some of the filters on an experimental basis in its nuclear test reactor at Pleasanton, California. Early in 1964, the Nuclear Energy Division accepted responsibility for development of the business, with H. W. Alter and S. C. Furman playing key roles. The new plastic filter is now produced and marketed under the trademark "Nuclepore."

The basic work on etched particle tracks also led to other significant opportunities:

*Geochronology.* Natural materials contain fission tracks, the result of trace uranium impurities, some of which spontaneously fissioned during geological time, creating in this way a record of the age of the samples.

*Solidification of celestial matter.* Tektites are small glass objects that have passed downward through the earth's atmosphere at hypersonic velocities and have landed on certain limited areas of the earth. Fission-track dating has shown the various ages of the four large impact areas, and that the time of arrival on earth is not measurably different from the time of original solidification.

*Age of fossil primates.* An application of fission-tracking dating of pre-history is the dating of volcanic glass from Bed I of the Olduvai Gorge, Tanzania, the site of the finds of two varieties of hominid remains. Fission-track dating gave a value of  $2.03 \pm 0.28$  million years, in agreement with the potassium-argon method which gave an age of 1.8 million years.

*Meteorites and cosmic rays.* Cosmic-ray tracks have been seen in meteorites. Because of their extreme antiquity and their long exposure to cosmic rays, meteorites contain a wealth of information about the early history of the solar system and about cosmic radiation. Much of this information is just beginning to be extracted.

*Discovery of Element 104.* During the course of his research, Price gave an oral report on his fission-track studies during a conference in Monterey, California. At the meeting were Russian scientists, and Price learned that they had begun research on particle tracks soon after the

appearance of the early published work on etched tracks. A year later (1964), Russian scientists from Dubna, a nuclear research institution near Moscow, announced that with the use of etched particle track detectors they had discovered atomic Element number 104.

*Nuclear physics.* The list of applications includes a new, more sensitive technique of neutron radiography and a method of performing alpha radiography in the presence of an intense background of beta and gamma rays. These developments were achieved principally by the Nuclear Energy Division's laboratory, after the division had taken over responsibility of the Nucleopore filter. In short, etched particle tracks are the basis of a new business in nuclear photography. Also, as a new technique of radiochemical analysis, e.g., in bio-analysis for plutonium, etched particle tracks are creating other business opportunities in the form of cost savings and process improvements.

### Key Personnel

- R. M. Walker:* BS (physics), Union College; PhD (physics), Yale University; Research Assistant, Yale University. GE, 1954-1966. Presently Professor of Physics, Washington University.
- P. B. Price:* BS (physics), Davidson College; MS and PhD (physics), University of Virginia. GE, 1960. Presently physicist (radiation effects in solids, electron microscopy, nuclear reactions, geochronology), GE Research and Development Center.
- R. L. Fleischer:* BA and MA (engineering science and applied physics), PhD (applied physics), Harvard University; Teaching Fellow, Harvard. GE, 1960. Presently physicist (irradiation effects in solids, geochronology, space physics), GE Research and Development Center.
- S. H. Seal:* MD, Rush Medical College; Memorial Sloan-Kettering Cancer Center, New York City.
- H. M. Rozendaal:* MD, University of Leiden. GE, 1947. Presently Medical Consultant, GE Research and Development Center.
- E. Fontanella:* Amsterdam School of Commerce, State University of New York at Albany. GE, 1950. Presently Laboratory Technician (friction, lubrication, adhesion, wear), Reactions and Structures Branch, GE Research and Development Center.
- S. C. Furman:* BS, PhD (chemistry), University of California at Los Angeles. GE, 1951. GE Nuclear Energy Division Laboratory.
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### **Lucalox® Lamp**

In 1954, it was decided to enlarge the Research Laboratory's effort on ceramics. The motivation for this decision was the belief that this important class of materials, having had ancient origins and a long history of empirical practices, might yield exceptional opportunities for the application of science-oriented concepts and techniques. One of the

major topics selected for study was sintering, the process of heating powders of material—typically oxides—until they stick together in a strong, dense mass. Sintering is one of the principal techniques of fabricating ceramic bodies, and is therefore an important industrial process. Moreover, large areas of analogy in metallurgical research were evident, for sintering has long been an active area of investigation among metallurgists; major fundamental questions remained unanswered about the atomic process that occurs during sintering of metal powders. Much less detailed work had been done up to that time on the sintering process on ceramic powders. J. E. Burke, who was in charge of ceramic research began the investigation by selecting aluminum oxide as the material for study, because alumina was available in pure form, and was an important representative material that might serve as a model for many other ceramics.

During sintering, particles weld together at temperatures far below their melting point. Many of the pores remaining after partial welding gradually disappear on further heating, but the end product always had considerable residual porosity. Burke's specific investigation was to find out how the pores disappeared during sintering and why some residual porosity always remained.

In 1955, R. L. Coble joined Burke's group and also began studying the sintering process. The work was unusually significant from a scientific point of view, and added considerably to the understanding of sintering in ceramics. The results of these studies were published in professional journals and presented at technical society meetings during the years 1955 through 1960.

In the course of Coble's experiments he had prepared some specimens of sintered alumina that were more dense than normal with a small degree of translucency. Early in 1956, G. E. Inman, manager of advanced engineering at GE's Large Lamp Department (Nela Park, Cleveland, Ohio), saw these specimens. He remarked that such material, if it could be made sufficiently transparent, might have advantages over fused quartz—the best materials then available for the envelopes of high-temperature lamps because of its ability to withstand higher temperatures. Some experiments had been done with sapphire—single crystals of alumina—drilled out to form lamp envelopes; while the results had been encouraging, the costs had been prohibitive. It was concluded that the potential utility of synthetic alumina ceramic in the field of light production warranted further research.

Coble discovered that small additions of certain other materials to the aluminum oxide powder improved transparency. By adding some magnesium oxide he produced reasonably translucent sintered material. Eventually, he found that by going to unusually low proportions of mag-

nesia, a fraction of a percent, he could obtain a sintered ceramic more nearly transparent than had ever been hoped for.

In April 1957, a group of three scientists—P. D. S. St. Pierre, E. R. Stover, and A. Gatti—began to develop and refine the process further. They devised a simpler process for making dense, translucent alumina and defined the processing conditions with more precision.

The direct product of this search was the production of an entirely new type of material; namely, a pore-free polycrystalline ceramic with optical properties resembling glass, the microstructure of a metal, and the high-temperature resistance of a ceramic.

Late in 1957, N. Grimm came to the Research Laboratory on temporary assignment from the Lamp Division in Cleveland for the express purpose of learning the technique for making the new ceramic and carrying it back to Cleveland. But the manufacturing process still needed some refining; unwanted colors sometimes marred the material. A defect, dubbed "measles," consisting of clusters of small pores which did not disappear during sintering, also plagued the process. Thus, when Grimm arrived, C. A. Bruch of the Research Laboratory began working with him and the Lamp Division to improve the processing techniques. After some time Bruch, who was in a group responsible for developing promising research results to the point of acceptance by an operating component, learned how to avoid the "measles" and the unwanted colors.

The Lamp Division thus gradually acquired the information and experience needed to begin making "Lucalox," as the new material was named. During the last quarter of 1959, the material was announced publicly and the Lamp Division formally assumed responsibility for the product, including, of course, the job of finding additional applications and of marketing the material.

Meanwhile, quite a bit of work was being done on measuring the properties of this Lucalox material, not only its optical properties but its strength, electrical properties, radiation tolerance, and so on. Based on these results, a wide number of potential applications were explored by various operating components of GE as well as a few external organizations. Lucalox was tested as a high-strength insulator, as a microwave tube window, as bearing material, as a cladding for fuel elements, as a gauge window in high-pressure water systems, etc. In some of these applications it was found to be technically satisfactory but economically unjustifiable; although very cheap compared to sapphire, it was still expensive compared to conventional alumina ceramic. Nevertheless, since 1957 a variety of applications of Lucalox have been found and the market for the product has steadily grown.

However, as noted before, one of the most striking properties of Lucalox was its ability to transmit light; it transmits at least 90 percent of the light striking it. Could it be used as one of the materials for

making a new light source? Its melting point was  $2050^{\circ}\text{C.}$ , very much higher than fused silica (quartz), a common lamp envelope material. Possibly in lamp use, it would not be subject to crystallization, one of the causes of failure of fused-quartz lamp envelopes operated under extreme conditions.

In its continuing search for new, more efficient light sources, the Lamp Division began experimenting with the new material in 1958. The type of lamp envisaged was one in which light is produced by an electric discharge through metallic vapor. It was hoped that with a strong envelope that could withstand higher temperatures a lamp could be designed with higher efficiencies than ever before. By 1960, K. Schmidt had discovered that sodium vapor could be used in a new way with the Lucalox ceramic material, and at higher temperatures than ever before, to achieve a highly efficient lamp giving a "golden" white light. This new invention gave great promise of a new light source for the future. W. C. Loudon worked extensively on lamp components: the metallic seals and electrodes; the sealing techniques for bringing the electric current into the interior of the Lucalox ceramic envelope for generating the electric discharge in the sodium vapor. All of this work was carried out in Cleveland with only intermittent consultation, especially on materials problems, with Research Laboratory.

In December, 1962 the new lamp was announced. Its characteristics were highly promising: efficiency, 105 lumens of light per watt of electric power, the most efficient source of light acceptable color in the history of electric lamps. Not satisfied with this initial success, scientists at both the Research Laboratory and Lamp Division continued their efforts to increase the reliability and life of the lamp and to ensure a lamp design of stable color characteristics fully amenable to repetitive processes of manufacture. Moreover, the manufacturing processes—those for making Lucalox tubing itself, those for sealing metal to the ceramic, and others—were enough different from widely used lamp manufacturing operations that they also required considerable development.

Accordingly, several scientists at the Research Laboratory—R. J. Charles, P. J. Jorgensen, and later in 1965, R. E. Hanneman—began working part-time on improvements in the materials and processes for the lamp and also on a fundamental analysis of the thermodynamics and kinetics of the chemical reactions occurring in the lamp. Based on this work and the continuing work of Schmidt and Loudon in Cleveland, the problems posed by repetitive production and by reliable operation for a sufficiently long life were gradually resolved.

In October 1965, it was announced that the lamp would be placed on the market in January 1966. Since that time, quantity manufacture has begun and Lucalox lamps are being sold. As with any new product and new manufacturing process, not all troubles are overcome during



the first few months of operation. Thus the research and development people, both in Cleveland and in Schenectady, continue to follow events, to suggest improvements, and to consult on problems. But there is no doubt that a major new source of light—comparable in importance to incandescent lamps and to fluorescent lamps—has arrived on the market from its modest beginnings in research on sintering.

It is important in the study of this case history to observe some of the factors associated with the environment and motivation for research that led to the successful final results. Research in light production, both in the central research establishment of the Company and in the laboratories of the Lamp Division, has been a major effort since the founding of the Company. Many major advances in the efficiency, utility, and economy of light sources have come from this work. Scientific study of an important class of materials—oxide ceramics—resulted in a new material, substantially transparent sintered alumina, which turned out to have a variety of uses. Close interaction between scientist and engineer, with mutual creative cooperation provided the fertile ground on which the seed of the one new idea brought forth a second idea in a new lamp which utilized the new transparent alumina as part of the lamp design. It seems certain that, without excellent communications across the scientist/engineer interface, and a mutual motive for creative progress, the Lucalox lamp would not have been born.

### Key Personnel

- J. E. Burke:* BA (chemistry), McMaster University; PhD (chemistry), Cornell University; Faculty, University of Chicago. GE, 1949. Presently Manager, Ceramics Branch, GE Research and Development Center.
- R. L. Coble:* BS, Bethany College; ScD (ceramics), Massachusetts Institute of Technology; Research Assistant, Massachusetts Institute of Technology. GE, 1955–1960. Presently Professor of Ceramics, Massachusetts Institute of Technology, Cambridge, Massachusetts.
- G. E. Inman:* BS (chemistry), Colgate; Western Reserve; Assistant Professor, Colgate. GE, 1919. Retired 1960.
- P. D. S. St. Pierre:* BS and PhD (metallurgy), Royal School of Mines, University of London. GE, 1955. Presently Manager, Engineering, Diamond Products Business Section, Metallurgical Products Department, Detroit, Michigan.
- E. R. Stover:* BS, MS, and PhD (metallurgy), Massachusetts Institute of Technology. GE, 1955. Presently Consultant, Ceramic Engineering, Re-Entry Systems Department, Philadelphia, Pennsylvania.
- A. Gatti:* BS (physics), Union College; MS (metallurgy), Rensselaer Polytechnic Institute. GE, 1950. Presently metallurgist, Space Sciences Laboratory, Philadelphia, Pennsylvania.
- N. Grimm:* BS (chemical engineering), Fenn College. GE, 1948. Presently chemical engineer, Applied Engineering, GE Glass Technology Department, Cleveland, Ohio.
- C. A. Bruch:* BS and MS (metallurgy), Pennsylvania State College; PhD (metallurgy), University of Pittsburgh. GE, 1952. Presently metallurgist at Space Sciences Laboratory, Philadelphia, Pennsylvania.
- K. Schmidt:* PhD (physical chemistry), Justus Liebig Hochschule, Germany. GE, 1957. Presently engineer, Advance Engineering, Large Lamp Department, Cleveland, Ohio.

- W. C. Loudon*: BS (electrical engineering), Case Institute of Technology. GE, 1942. Presently engineer, High Intensity Discharge Engineering, Large Lamp Department, Cleveland, Ohio.
- R. J. Charles*: BS (mining) and MA (metallurgy), University of British Columbia; ScD (metallurgy), Massachusetts Institute of Technology; Assistant Professor, Massachusetts Institute of Technology. GE, 1956. Presently ceramist (structure and properties of glass, phase separation and conduction mechanism of glass, communication), GE Research and Development Center.
- P. J. Jorgensen*: BS (geology), Brigham Young University; PhD (ceramics), University of Utah. GE, 1960. Presently ceramist (sintering, diffusion, solid state reactions), GE Research and Development Center.
- R. E. Hanneman*: BS (metallurgy), Washington State University; Whitworth College; MS and PhD (metallurgy), Massachusetts Institute of Technology. GE, 1963. Presently metallurgist (thermodynamics of phase changes, high pressure, microprobe and X-ray techniques), GE Research and Development Center.

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### Lubrication

Lubricants are important to GE because the company makes a wide variety of rotating equipment, including motors, generators, turbines, and aircraft jet engines. Also, because almost every manufacturing plant uses lathes, milling machines, and other metal-removing machine tools, the effectiveness of lubricants used during the metal-removing process—when the cutting tool bites into the metal—is important to efficient operations. Although friction, wear, and lubrication have been technical subjects of primary interest to the company for a long period of time, and many serious problem areas have been known, no basic research in the field was done at the Research Laboratory prior to 1958. Work elsewhere had

been followed and a certain amount of fundamental work in the field at Cambridge University, England, had been partly supported by GE. These investigations were excellent scientific endeavors, but very little of a practical nature had come from them. On the opposite side of the picture, a substantial sum of money was being spent by oil companies in the United States and elsewhere on empirically formulating new lubricants for special uses. But, for the most part, fundamental science had not been closely connected with improvements in lubrication in areas of interest to GE.

Because of this situation, the laboratory management decided that a program was warranted on friction, wear, and lubrication that would try to span the gap between fundamental science and practical problems. In 1958, this program was begun. An interdisciplinary group—including inorganic, organic, and surface chemists, and two mechanical engineers—was assembled. Individuals whose interests ranged from quite fundamental studies to rather practical matters were included. As the effort began, steps were taken to heighten its contact with both science and practice. Several scientists from academic institutions, who were interested in aspects of the field, were invited to spend their summers at the laboratory. At the same time the manager of the project and a mechanical engineer toured many operating components of the company, discussing the problems of friction, lubrication, and wear encountered by equipment designers and manufacturing people. Several other companies and Government installations also were visited.

Meanwhile, the fundamental scientists in the group were not instructed to work on any of the obvious or even most pressing problems of lubrication. Rather it was hoped that the discussions with visiting scientists and the infusion of fresh thought by novices in the field might result in recognizing some new fundamental approaches to the subject.

L. E. St. Pierre, an organic chemist in the group, began one line of attack. He argued that since the wear process of two metal surfaces sliding against one another involves microscopic protuberances that weld together and then shear apart, they create—momentarily—fresh metal surfaces. Thus, he felt one might learn something of importance for wear and lubrication by studying chemical reactions at the surfaces of fresh metal. However, ordinarily the metal surface produced by, say, cleaving a piece of aluminum in air, would result in an oxide layer covering the “fresh” surface in a billionth of a second. Thus, St. Pierre decided to use ultrahigh vacuum techniques as a means of slowing down the progress of the surface reaction; he could produce fresh metal by evaporating a film of it in an ultrahigh vacuum and keep it fresh long enough to introduce and observe the reaction of substances he wanted to study. Aluminum was the first metal chosen because of the substantial existing knowledge of its chemistry, because it was by reputation a

particularly difficult metal to lubricate, and because the discovery of methods to lubricate this metal would thus have important practical consequences.

In 1960, St. Pierre found that certain organic chemicals—simple olefins such as ethylene—attach themselves to clean aluminium surfaces very rapidly and hold there tenaciously even when heated to 100° C. This discovery proved to be a landmark in the program.

Meanwhile, R. S. Owens and R. V. Klint, the mechanical engineers in the group, had begun to emerge as among the company's leading experts in the practical problems of lubrication. They had built experimental equipment for testing lubricants in the laboratory, had maintained their contacts with operating components, and through their consultation had established good working relations with key people in the operating components. They had, indeed, developed new ideas of their own for improving lubricants. What they learned from the fundamental studies, even before the crucial discovery, also had affected their thinking.

The ground was, in a sense, so perfectly prepared for a key event that within a week of St. Pierre's fundamental discovery, it had been transformed into the invention of a new lubricant for aluminum. The basic idea was to choose an organic molecule that, like St. Pierre's olefins, would attach itself tenaciously to a fresh metal surface (later it was shown that for aluminum the molecule also had to be designed to attach itself to tiny aluminum oxide particles which occur and must be "lubricated" to avoid their abrasive action) and which would have a long chain of atoms to provide the lubricating "cushion" between the surfaces. By mid-1961, St. Pierre, Owens, and Klint had demonstrated the practical feasibility of adding the new lubricant molecules to the normal lubricant of a fractional-horsepower motor, running steel shafts directly on aluminum motor casings.

In retrospect, one could debate whether the invention of this new class of lubricants would have been made without the key knowledge gained by ultra-vacuum studies. Speculation on this "iffy" question is far less instructive than a focus on the intensely stimulating and creative setting for progress which was achieved by bringing together a group of workers with both fundamental and applied interests—with daily opportunities for the cross-fertilization of ideas and the exploration of concepts. In fact, to those near this program, it is difficult to imagine how the final successful results could have been achieved in this field without the symbiotic relationship of both basic and applied lines of investigation.

The developing association of Owens and Klint with key people in the operating components greatly aided the subsequent events. GE's Appliance Motor Department at De Kalb, Indiana, began engineering studies of the use of the new lubricants in their product. They found it would be advantageous to completely redesign their motors, and in 1965 the new

line of motors utilizing the lubricants was introduced. Also in 1961 and 1962 members of the new family of lubricants were introduced into metalworking fluids used in the company for aluminum wire-drawing.

Patents covering the new lubricants were applied for. In 1963, through GE's Patent and Technology Marketing Operation (a component established specifically to license and sell patents and technology to other corporations), the patents were licensed to a lubricant manufacturer, the E. F. Houghton Company, which now markets a line of products based on these inventions.

The approach to lubrication that was so successful with aluminum—labeled the “chemical approach to lubrication” because it sought useful chemical reactions with fresh, uncontaminated metal surfaces—did not appear limited to the type of compounds used for aluminum. Thus, in 1962, spurred by the same general approach, R. W. Roberts, who had joined the group in 1960, postulated that certain lamellar compounds of metals with iodine might be good lubricants for the metals. Many important and difficult-to-lubricate metals—titanium, iron, cobalt, and nickel—form these lamellar iodine compounds. However, iodine itself is a brittle, solid material unsuitable for use as a lubricant. Thus, two steps were needed: first, to find a way of introducing iodine at the fresh metal faces produced during sliding; second, to prove that the compounds predicted to form would indeed lubricate. Roberts and Owens found that iodine dissolved in certain organic chemicals, forming an “iodine charge transfer complex,” could be used as a lubricant either of itself, or preferably, added to oil. This type of lubricant proved to be far better than any prior lubricant for both rotating equipment and metalworking applications involving titanium, stainless steels, superalloys, and many other materials that had always presented serious problems of lubrication.

A patent covering this family of lubricants issued in 1966. By the end of 1966, the lubricants were being used in many experimental and testing applications throughout the company. Iodine itself is highly corrosive to ordinary machine tools, and special procedures are presently needed to use the lubricant in metalworking operations. Further development of practical lubricants and cutting fluids is continuing in a joint project between the research staff and an operating component. Also, because of the potential importance of this approach for the fabrication and use of aerospace materials—titanium, superalloys, stainless steels—work has begun under a Government contract to extend and develop the knowledge and use of these lubricants. Thus, the second chapter of the lubrication story is still only partially written.

Nevertheless, the value of an approach that combined fundamental studies based on modern ultrahigh vacuum techniques, with thorough knowledge of the practical barriers in a field like lubrication, has been proved by the results.

### Key Personnel

- L. E. St. Pierre:** BS (chemistry), University of Alberta; PhD (chemistry), University of Notre Dame. GE, 1954. Presently Professor of Polymer Chemistry, McGill University, Montreal, Canada.
- R. S. Owens:** BS (marine science), Maritime College, State University of New York; ME (mechanical engineering), Rensselaer Polytechnic Institute. GE, 1953. Presently mechanical engineer (friction, lubrication, wear studies), GE Research and Development Center.
- R. V. Klint:** BS (mechanical engineering), Cooper Union School of Engineering; PhD (mechanical engineering), Rensselaer Polytechnic Institute. GE, 1945. Presently in Laboratory Operation, GE Power Transmission Division, Philadelphia, Pennsylvania.
- R. W. Roberts:** BS (chemistry), University of Rochester; PhD (physical chemistry), Brown University. GE, 1960. Presently Manager, Structures and Reactions Branch, GE Research and Development Center.

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### **Lexan®**

The discovery of Lexan polycarbonate resins—a family of engineering polymers with high impact strength and resistance to high temperatures, water, and acids—is an example of that much-used term, "serendipity."

In 1948, the Research Laboratory launched a program to discover an improved material for thin-film magnet wire insulation. Such insulated wire is a critical part of electric motors and other apparatus manufactured by GE. The search was for a material to withstand high temperature—at least 150° C.—because a high-temperature wire insulation permits higher performance in a smaller motor size.

By early 1953, a family of polymers that looked promising had been discovered. One of the investigators was D. W. Fox, who was on a six-month orientation assignment at the Research Laboratory from the Materials and Processes Laboratory of the company's Large Steam Turbine-Generator Department, Schenectady. Fox had joined the Materials and Processes Laboratory in August 1953, after doing postdoctorate work at the University of Oklahoma.

The practice of having newly employed professional personnel from the Materials and Processes Laboratory assigned to the Research Laboratory was a unique and rewarding program. The program's purpose was to broaden the outlook and knowledge of the men by having them work with and meet laboratory personnel, and become better acquainted with the various projects and facilities. Such knowledge would help them when they returned to their regular jobs by making them more aware of sources of counsel and advice that were available to them during the course of future investigations.

Although Fox had no experience with polymers, he entered wholeheartedly into the investigation. At that particular point in time a family of polymeric materials had been discovered that had the proper flexibility, toughness, and resistance to high temperature, but all were somewhat degraded by water and none had been found that could be applied to wire fast enough to be useful.

During a review session of Research Laboratory chemists on the wire insulation project, they compared notes on progress. Degradation by water was still a problem. The thought occurred that instead of trying by small changes to improve the hydrolytic stability of the polymer family being studied, perhaps it would be better to start with groups that were hydrolytically stable and synthesize these into a polymer with the other requisite properties for wire insulation. This suggested a reverse approach and asked a rhetoric question: "Does anyone know of any

hydrolytically stable ester?" This question triggered a flashback in Fox's mind, back to his postdoctorate work at the University of Oklahoma.

He recalled that for an experiment he needed a particular chemical—guaiaicol—but it was not in stock and he couldn't get it immediately. However, he was told that the school had drums of army surplus guaiaicol carbonate and that he could have all that he wanted. Fox recalls: "I assumed it would be a very simple matter to take some of this and put some caustic or acid with it and boil it for a few minutes and tear it apart. However, after several days of boiling, I still had not decomposed any of it, and I gave up. It was this connection that came to my mind when we were looking for a method of making a polymer which could not be torn apart by boiling water or by steam. I recalled the guaiaicol carbonate, and decided to try to make a polymer based on this material."

His first approach was to try a bis-guaiaicol compound, but there was none in the laboratory stockroom so he searched for an analogous compound—and selected bisphenol-A (this is a basic material for making epoxy resins).

Here is how Fox described what happened: "I tried it and started making a polymer by ester-exchange with diphenyl carbonate and the melt became more and more viscous. Eventually, I could no longer stir it. The temperature had reached about 300° C., and I stopped at this point when the motor on the stirrer stalled. When the mass cooled down, I broke the glass off and ended up with a 'mallet' made up of a semi-circular replica of the bottom of the flask with the stainless steel stirring rod sticking out of it. We kept it around the laboratory for several months as a curiosity and occasionally used it to drive nails. It was tough!"

What Fox had discovered was, basically, the material that later was to be called "Lexan."

(Fox was asked recently if he had thought of any commercial use for a plastic exhibiting the high impact strength and resistance to high temperature of his "mallet." He replied: "At the time I had the glob on the end of a stirring rod the main objective was to make a wire insulation. I personally felt that we should have worked to modify this glob into a better wire insulation. The people heading the project, however, felt they were very close to a useful product in the research underway and wanted to finish it. In fact what proved to be the last piece of the jigsaw puzzle was discovered within days of the polycarbonate discovery. My own comprehension of the plastics industry was very definitely limited at this time. I didn't know anything about it, so that I personally would have been in a poor position to have pushed to get this product developed commercially.")

Early in 1954, A. Pechukas visited the Research Laboratory and saw the odd-looking polycarbonate "hammer." At that time he was general manager of the Chemical Development Department of GE's Chemical



and Metallurgical Division, Pittsfield, Massachusetts. Pechukas became very interested in Fox's discovery. Having just concluded an investigation, he was anxious to bring a promising new project into his department.

Pechukas urged the Research Laboratory to investigate Fox's discovery, but immediate action was not possible because the pursuit of a successful material for wire insulation was reaching a climax and all energies were devoted to that goal. The goal was achieved when Fox, with co-worker F. M. Precopio, invented the wire-insulation material that is called Alkanex®. (This insulation discovery was a polyester—tough, flexible, and stable. Polycarbonates proved of little value as wire insulation.)

Fox returned to his job at the Materials and Processes Laboratory and soon after was visited by three scientists from the Chemical Development Department who wanted all available information on the new polycarbonate material. A program was launched at Pittsfield, and simultaneously Fox set up a small, independent program in his laboratory, spending about one-half of his time on it. "Fortunately," Fox has since remarked, "my boss was very understanding."

Several promising process techniques were developed by both groups. At the end of 1954, Fox was offered a position on the polycarbonate project team at Pittsfield, and in January 1955 he moved to his new position.

In this particular case history there was no "formal" transfer of the polycarbonate invention from the Research Laboratory to the Chemical Development Department. That department was the logical place in which further development work could be carried out, with a formal transfer to the Chemical Materials Department if the work was successful.

The Chemical Development Department, now called Chemical Development Operation (CDO), is rather unusual in GE. The finances and management are isolated from profit-and-loss worries and day-to-day "firefighting." CDO is a new-product incubator, taking the idea from conception to the point where a full-fledged business could be launched. It has its own marketing staff, as well as scientists and production people. If the final product will not "fit" within the scope of an established product department, a new department may be created to manufacture and market the material.

Within the Chemical Development Department, the decision was made to develop a polycarbonate resin to be used as a molding material—this was a critical step because it was GE's first attempt to enter a new material of its own in the thermoplastic molding material business. Work progressed on the development of a polycarbonate of high molecular weight. This proved to be a false trail. "We were the victims of some bad marketing information," Fox has since said. "We wasted six months

trying to get a product of high molecular weight when a product of one-half the molecular weight we thought we needed would have done the job. We just didn't have the marketing facts of life."

The initial trials of the polycarbonate resin on commercial molding equipment were unsuccessful, "almost to the point of killing the project," R. J. Prochaska, who was deeply involved, said recently. "The product, as molded, looked more like burned toast than a usable material."

After discovering that a material of a lower molecular weight was acceptable for commercial use, and refining the process to make it possible to use conventional pilot-plant equipment, the project was back on the track.

Formal announcement came in 1958 when the pilot plant was producing enough resin for test marketing and field trials.

In late 1957 and early 1958, GE management decided that it must either invest large sums in full-scale production or drop the project. The decision had to be made before there was assurance that the product could be made at economic cost and quantity, and before demonstration of any large-scale market demand. Management also was aware that if market demand followed predictions, it would quickly outstrip available pilot-plant production.

The decision was affirmative, and in January 1959 there was official transfer of the Lexan project (as it had been named in 1956) from the Chemical Development Department to the Chemical Materials Department. Not only was the product transferred, but also the project team that had nursed it along from the beginning.

Transferring the manpower with the product helped reduce the start-up time, and made operations smoother. Both Fox and Prochaska agree that the team was "totally committed to make the project successful . . . everyone was deeply involved because their own futures were involved."

The management decision to go full-scale on Lexan production led to the construction of a new facility on a 162-acre tract along the Ohio River near Mt. Vernon, Indiana. The \$11 million plant went on-stream in October 1960, with an output of 5 million lb per year. Since that time, production has been increased manyfold.

The project was moved from pilot plant to commercial success in less than three years.

Three further points on the development of Lexan polycarbonates are worth noting:

1. Since Fox's initial discovery, literally thousands of other bisphenolic compounds, singly and in combinations, have been tried. To date, chemists have not been able to produce a product with a better *over-all* balance of characteristics than that produced by Fox with the original bisphenol-A. The workhorse of Lexan production continues to be bisphenol-A.

2. Thirteen years ago, if one had asked the average chemist what he thought of the possibility of making a polycarbonate resin, he probably would have replied, "You can make it, but it would fall apart in water." The general consensus; as described in textbooks and the chemical literature, was that polycarbonates were unstable in the presence of water. "If you read the chemical literature, and believed everything, we would never have made Lexan," Fox says.

3. Although the basic invention was made 13 years ago, chemists still don't know *why* this polycarbonate has the properties it does, or why it performs the way it does. "We have made the color of the material lighter, we have made it easier to produce, but we still have a long way to go to really discover what it is," Prochaska said recently.

### Key Personnel

- D. W. Fox*: BS (chemistry), Lebanon Valley College; MS and PhD (organic chemistry), University of Oklahoma. GE, 1953. Presently Consultant, Chemical Development Operation, GE Chemical and Metallurgical Division.  
*A. Pechukas*: BS and PhD (chemistry), University of Chicago. GE, 1950. Presently in Laboratory Operation, GE Component Products Division, Fort Wayne, Indiana.  
*R. J. Prochaska*: AB (chemistry), Dartmouth College; MS (chemistry) and PhD (organic chemistry), Rutgers University. GE, 1951. Presently Manager, Polycarbonate Research and Development Section, GE Chemical and Metallurgical Division.

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### PPO ® Polymer

In 1955, A. S. Hay, upon completing his graduate work, joined J. R. Elliott's Organic Chemistry Section of the Research Laboratory as a chemist. While in school, Hay had in mind some general ideas for direct oxidation of organic compounds. He was encouraged to try out his ideas after joining GE.

The eventual result was a new chemical technique for synthesizing plastics. Hay's early work had its share of disappointments, but there were enough positive results to keep him pushing forward. Along about the middle of the 1950's, as a result of study of the literature and translation of some of his own thinking on the subject, Hay conceived the idea for the oxidation of phenols using a cuprous salt in pyridine as a catalyst.

Although initial results with phenol were not up to expectations further work showed how he could tailor the reactions and in a short time he had obtained a polymer of a high molecular weight. The basic invention was made.

The discovery was "polymerization by oxidative coupling," in which oxygen from ordinary air plays a key role in making unique plastic materials. (The characteristics of one of these plastics will be described below.)

Following the basic discovery, scientists recognized that the monomer to make the polymer was both expensive and not too available. This monomer was 2,6-xylenol. Also, the final product failed under high temperatures. With these shortcomings, the material could not be used for insulation purposes. For that reason, the project was put aside and the scientists focused their efforts on phenol oxidation.

Liaison on the subject between the Research Laboratory and GE's Chemical Development Operation in Pittsfield Massachusetts, was continuous after Hay's discovery. Even though it held out promise as a commercial business, the Chemical Development Operation was reluctant to take on another major venture because the Lexan program (described in preceding section) was just under way.

However, in 1959, with Lexan on a sound footing, the climate was ripe for CDO to develop another major business. Also, the Research Laboratory's effort to obtain a phenol polymer by oxidative coupling was fruitless, so everything pointed to a thorough reconsideration of Hay's discovery. In 1960, the Chemical Development Operation and the Research Laboratory decided that the business potential was favorable.

Elliott, who was in charge of most of the work, saw that the key to an economic product using Hay's discovery would be to synthesize a monomer that would be inexpensive. He started a monomer synthesis project with S. B. Hamilton as a key contributor. Within a year's time the team came up with the answer. The project involved about one-and-a-half men; later a chemical engineer was added to improve the process for commercial production of the project.

As Elliott has since remarked, "This wasn't cookbook chemistry. This was all new. There just weren't any texts on the subject. We had to find out the chemistry of what was going on."

In 1962, one key scientist from the Research Laboratory, R. P. Anderson, was transferred to the development work in Pittsfield. Also some "research-training" personnel were transferred to the Chemical Development Operation; but that organization built its staff primarily on its own.

On December 1, 1964, GE announced publicly its interest in commercializing the technique of "polymerization by oxidative coupling." Of the wide variety of plastic materials that can be formed by this new technique, the first to be offered commercially was polyphenylene oxide

or "PPO." This is an extremely strong and durable material that exhibits good physical properties at temperatures about 370° F., far higher than any other readily moldable thermoplastic material. It can be injection-molded in conventional equipment, machined like brass, and extruded into rods, slabs, sheets, and pipes. Surgical instruments made of PPO can be sterilized repeatedly in an operating-room autoclave, and a variety of special surgical devices have been made for the U.S. Army. Other applications include insulation for extra-high-voltage transmission lines, battery cases, appliance parts, and other jobs requiring materials with excellent mechanical properties and durability over a wide range of temperatures.

Up to the time of the first public announcement of PPO, GE had invested approximately \$7 million in research and development activities. Pilot-plant quantities of PPO produced by the Chemical Development Operation at Pittsfield were available on January 1, 1965. Commercial responsibility had been transferred, of course, to the Chemical and Metallurgical Division some time before and R. Gutoff, manager of the Chemical Development Operation, spearheaded the development of PPO.

A plant to manufacture PPO resins has been built south of Albany, New York, and joint-venture companies are being established in The Netherlands and in Japan for the manufacture and sale of PPO.

### Key Personnel

- A. S. Hay*: BS and MSc (chemistry), University of Alberta; PhD (chemistry), University of Illinois; Instructor, University of Alberta. GE, 1955. Presently organic chemist (oxidation reactions, catalysis, polymer synthesis), GE Research and Development Center.
- S. B. Hamilton*: BS (chemistry), William and Mary College; PhD (chemistry), Northwestern University. Presently Manager, Personnel and Administration, General Chemistry Laboratory, GE Research and Development Center.
- J. R. Elliott*: BS (chemistry), Iowa State College; PhD (chemistry), University of Illinois. Presently Manager, Organic Chemistry Branch, GE Research and Development Center.
- R. P. Anderson*: BS, University of Notre Dame; PhD, Harvard University, GE, 1955. Presently Manager, Research and Advanced Development, GE Chemical Development Operation, Pittsfield, Massachusetts.
- R. Guoff*: BS, Columbia University. GE, 1948. Presently Manager, Chemical Development Operation, Bridgeport, Connecticut.

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### ***Ion-Exchange Membrane Fuel Cells***

The fuel cell is an example of a research result that was more than 100 years ahead of its time. Discovered in 1839 by Sir William Grove, it remained for many years a scientific curiosity—a way to make electricity by means of a chemical reaction that did not involve burning. The promise was intriguing, because the fact that no heat cycle was involved meant that the efficiency—theoretically, at least—could be extremely high. But making it work on a practical scale proved extremely difficult.

After World War II, interest in fuel cells, primarily for central station use, began to stir. The June 28, 1952, issue of *Business Week* described work being done by F. Gorin of the Pittsburgh Consolidation Coal Company. The article was noted by the Research Laboratory, and also by H. A. Winne, who was then vice president of engineering for GE (he has since retired). In a letter to the director of the Research Laboratory, Winne suggested that investigation of fuel cells would be a worthwhile activity.

GE was, of course, interested in protecting its interest in the power-generating field, so a modest effort began in 1953 under the direction of H. A. Liebhafsky, manager of the laboratory's Physical Chemistry Section. Work in this area progressed, and in 1955, a second man—W. T. Grubb—was added to the project. His contribution was significant, and was one of the two major breakthroughs that led to the successful use of fuel cells on the Gemini space program.

Grubb conceived the use of an ion exchange membrane as the electrolyte; he was the first to appreciate that certain polymers could be used as electrolytes for fuel cells. He continued his work to the point of making the invention and obtaining a patent (U.S. 2,913,511).

Grubb's discovery was an important one, but it was apparent that more work would have to be done to make the fuel cell applicable to central station power sources, which seemed the most likely goal at that time.

Work continued at approximately the same level of activity, even through the beginning of the Sputnik era. Early in 1958, Grubb was

transferred to liaison activities in the laboratory, and work on ion-exchange membranes came to a temporary halt.

But Sputnik *did* have its effect. Liebhafsky was convinced that fuel cells—and in particular the ion-exchange membrane type—would have wide application in the space program. In mid-1958, new effort was devoted to refining the approach using an ion-exchange membrane. L. W. Niedrach was added to the staff, and D. L. Douglas stopped his work on another type of fuel cell to concentrate on Grubb's findings.

The year 1958 saw the second breakthrough in fuel cell technology: Niedrach invented an improved electrode structure that solved the bonding problem between electrode and electrolyte. This resulted in a reliable design, and samples of the Grubb-Niedrach cell were built for demonstration to Government and company components.

All fuel cell research described to this point was financed by GE funds.

These men began an extensive life-test program to determine whether the development of ion-exchange-membrane cells had gone far enough to warrant the building of batteries—that is, power sources in which a number of fuel cells were connected in series to raise voltage. During 1959, partial Government sponsorship of the work in the Research Laboratory began. This support came through the company's Missile and Space Division with two objectives: the study of a "regenerative system" for use in space, and the building of an ion-exchange-membrane battery. The first objective was a power source in which electricity derived from solar energy in space would produce hydrogen and oxygen during the orbital day, these gases being reunited in a fuel cell to produce electricity during the orbital night. This work by Niedrach, though promising, could not be carried through to a practical device. Meanwhile, E. J. Cairns and Douglas designed, patented, and built batteries (power, 15 watts) and laid the foundation for further battery development in GE's operating components.

During 1958 and 1959, Liebhafsky had contacts with W. C. O'Connell, general manager of GE's Aircraft Accessory Turbine Department (AATD), West Lynn, Massachusetts. O'Connell was looking for new products for his department to bolster the market for his conventional products. "Unconventional power sources" looked like an attractive—and major—product line. Liebhafsky emphasized in his discussions with O'Connell that having a "mechanical engineering organization convert to an electrochemical organization was difficult to do."

AATD in 1959 won a contract from the Signal Corps for a 200-watt portable power source. This was the first major product contract in the fuel cell field in GE. AATD personnel were trained at the Research Laboratory to expedite the beginning of the work on the fuel cell in West Lynn. To further smooth the transition, Douglas transferred to AATD in 1960.

During 1960–1961, work at AATD concentrated on over-all refinements and improving the membrane.

In 1962, the fuel cell activity of AATD was renamed the Direct Energy Conversion Operation (DECO) as a part of the company's Defense Electronics Division, with a 1962 budget of \$4 million. The organization of a component of this size devoted almost entirely to fuel cells was a landmark in fuel cell history.

Also in 1962, DECO was awarded the contract for the Gemini fuel battery. The application was successful, although the engineering problems posed by limitations on weight volume, and reliability were drastic.

The transition from a research development to production to meet the challenge of the Gemini program was remarkably smooth. Three key factors helped make it successful:

1. The vision and faith—and enthusiasm—of O'Connell in the future of fuel cells when they were not much more than working models. This eagerness on the part of O'Connell and the men of his department to assume responsibility for the project, and the extent to which they had developed the fuel cell from its original laboratory version, were important factors in making a strong start toward what was to be the world's first major practical application of fuel cells.

2. The early transfer of Douglas to AATD, where he became a key member of the fuel cell effort. In this way, knowledge and experience gained in the Research Laboratory became effectively available to an operating component of the company.

3. The application of organizational, developmental, and production know-how by the Direct Energy Conversion Operation team under its second manager, R. S. Mushrush. The satisfaction indicated by the National Aeronautics and Space Administration officials during and after seven Gemini flights powered by fuel cells shows that, given eagerness and a good technology, transition to a practical product can be smooth even though the scientific field was originally strange to the men who applied it.

This case history has concentrated on only one type of fuel cell so as to maintain clarity and a continuous theme. Actually, as early as 1959, work on other types of fuel cells was carried out by the Research Laboratory. In 1960, for example, fuel cell research at the laboratory was raised to a six-man level, on GE funds. Work advanced on other designs, including one using low cost hydrocarbons (such as propane) as a direct fuel instead of hydrogen. While the Gemini program was still underway, it was logical to look toward the future and to examine how this newly important technology might be used in fields other than space. This has led to the application of ion-exchange membrane fuel cells to underwater buoys capable of unattended operation for long periods. Of even greater importance in the future may be the use of ion-exchange



membrane fuel cells in ground applications including military, industrial, and perhaps even some consumer uses. GE continues to invest in these efforts—an example of how an imaginative approach, based on the vision of wide use of a new technology, has carried it from basic invention to the stage where it may well be applied to a variety of the industry's products.

### Key Personnel

- W. T. Grubb*: SB (chemistry) and PhD (physical chemistry), Harvard University. GE, 1949. Presently physical chemist (fuel cells, batteries, electrochemistry, ion-exchange polymers), GE Research and Development Center.
- H. A. Liebhafsky*: BS (chemical engineering), A and M College of Texas; MS (chemistry), University of Nebraska; PhD (chemistry), University of California. Faculty, University of California. GE, 1934. Presently Manager, Electrochemistry Branch, GE Research and Development Center.
- L. W. Niedrach*: BS (chemistry), University of Rochester; PhD (chemistry), Harvard University. GE, 1948. Presently physical chemist (fuel cells, electrochemistry, fused salts, inorganic chemistry, solvent extraction), GE Research and Development Center.
- E. J. Cairns*: BS (chemistry, chemical engineering), Michigan College of Mining and Technology; PhD (chemistry), University of California. GE, 1959–1966. Presently with Argonne National Laboratory.
- D. L. Douglas*: BS and PhD (chemistry), California Institute of Technology. GE, 1951–1964. Presently with Gould National Batteries, Inc.
- W. C. O'Connell*: BS, Rensselaer Polytechnic Institute. GE, 1954–1964. Presently engaged in management consulting field.
- R. S. Mushbrush*: BS (electrical engineering), University of Iowa. GE, 1941. Presently Manager, GE Direct Energy Conversion Operation, West Lynn, Massachusetts.

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### A Comment on Semantics

The case histories just summarized show, first of all, the futility of trying to label various elements of the research and development process as "basic," "applied," or "development." Given almost any definition of these terms, one can find variances or exceptions among the examples. Was the search for understanding of the sintering process in alumina (conducted by a scientist who had no intention of making a better lamp) any more "pure" than the studies of chemical catalysis conducted by the research team which was admittedly trying to make diamonds? Was the building of the first experimental vacuum switches merely "development," when success depended upon continuing to learn new basic information about gases in metals, high vacuum, and the behavior of arcs?

The processes of generating and utilizing science are so diverse and intricate that one gains an incorrect impression of them when they are described by terms that are too limited.

### The Sources of Applied Research

The case histories that have been outlined include a variety of sources for the "applied" work. These nucleating events fall generally into three categories:

1. The need or opportunities of the *marketplace* can be a source of these projects. The sales potential for a new source of industrial diamonds

was fully recognized before the all-out research program was undertaken. Research leading to "P-Zero" insulation was initiated because of presumed customer-want which, in the long run, did not materialize. A need for an improved wire enamel led directly to a solution (Alkanex) and indirectly to a completely new business (Lexan). It is apparent from the examples that, to be meaningful, the needs or opportunities of the marketplace must be reduced or translated into questions that are specific enough to be attacked by a research project. It is not adequate for example, that there is a need for "better" thermal insulation in refrigerators; the refrigeration engineer's analysis suggesting that the critical respect in which new thermal insulation must excel was in its space requirement constituted only part of the story. Bundy and Strong, by showing the effectiveness of evacuated panels (if they could be kept separated in a manner that maintained the insulating properties of a vacuum), provided insight concerning the explicit technical problem that needed to be solved. "Better" electrical insulation has been needed in the electrical industry since its inception, but the specific need for a series of modern wire enamel properties permitted the explicit development of Alkanex and the fortuitous discovery of Lexan. Generalized market conditions must be translated into rather specific terms describing the technical problems.

Some additional comments need to be made about the role of competition. Competition of the marketplace is a prime factor motivating the sponsor in all American industrial research. Each one of the ten cases discussed in this report had its basic roots in the fundamental objective of the sponsoring company to achieve profitable growth and maintain profitable business leadership. This company traditionally has "grown from within" much more than by acquisition; thus while ideas for improving *existing* products have always been essential, ideas for new products have received equal consideration. Great efforts are made to provide a climate of "urgent acceptance" for both.

Therefore, in the cases such as the tunnel diode, the vacuum circuit breaker, and the Lucalox lamp the motivation to innovate—and then to apply new ideas as rapidly as possible—resulted not from an actual announcement by a competitor but rather from of a continual apprehension that the competition is surely seeking new and better products, too. The examples of actual *research* programs triggered by a competitor's announcement are surprisingly rare in the company's history. (Examples of "catch-up" *development* activities are somewhat less uncommon.) None of the cases herein were initiated by a competitor's surprise announcement; rather, they all stemmed from a general—but extremely powerful—belief that only by innovating as fast as possible can long-term business leadership and growth be maintained.

2. Another source of applied projects is the actual *design or production* of products. Good engineering design or manufacturing processes may

encounter problems due to a lack of scientific knowledge of techniques. Thus problems with materials in the design of a product or with processes in its manufacture may stimulate investigations which may in turn develop not only improved methods and materials, but also new basic understanding. The impossibility of making good bearings out of aluminum—and the difficulty of deep drawing aluminum—provided motivation for lubrication research and led to new basic knowledge of this process. Long-held design ideas for using high vacuum for circuit interruption required—and prompted—the extension of research in vacuum techniques, seals, and outgassing of metals.

3. Another source of such “applied projects” is *invention* based on discoveries or on new knowledge emerging from fundamental research. The tunnel diode, Nuclepore filter, Lucalox lamp, and PPO polymer are all good examples. The vacuum switch is a variation insofar as the discovery of gas-free metals was concerned; in this case the new knowledge came from applied research—but in a completely unrelated field: semiconductor research.

The cases also illustrate that projects may begin with a decision at any level of the managerial hierarchy: some began through the initiative and decision of the laboratory director; some began with the insight and interest of individual researchers. It is important to have an organization in which all of these sources can be tapped or be the catalyst for new programs; neither the research staff nor the management can be narrow-minded or dogmatic about its prerogatives in initiating programs.

It is evident that organizational barriers and interfaces, particularly as they affect the flow of information, must be kept to a minimum not only in the research organization, but also in the components which it serves. This is even more necessary in large organizations than in small ones, for research on a broad front, serving a diverse technical clientele, generates a greatly expanded possibility of matching an industrial *need* to a technical *capability*. Thus the wire enamel failure, Lexan, became a highly successful structural polymer, and the “curious” new translucent alumina stimulated a far-reaching improvement in light production. In neither case was the important final result an explicit objective of the research. In each case the result was achieved through communications which permitted a match between an industrial *need* and a scientific *ability-to-do*. The perception of opportunities for new programs, whether suggested by the marketplace, by design and production, or by discovery and invention, is not a monopoly of any level in a healthy research establishment. The attitudes of those in the organization must reflect this fact.

Equally important, the scientific staff should have an unbiased attitude toward the sources of research and should be excitable over a challenging idea independently of whether it came from science itself, or from the

marketplace or production line. It is the *quality* of the idea that should matter, not the *source*.

### The Tactics of Applied Research

The timeliness of the opportunity to undertake an applied research program is exceedingly important. Although an application usually depends upon a single idea or a few key technical ideas, it also depends upon a large number of other contributing scientific and technical inputs. To be successful, the state of science and technology must be adequate to support the application in all of the contributing areas. Plunging into an applied project on the basis of a single scientific breakthrough when there is a weak link or an insuperable barrier elsewhere in the total picture is, of course, doomed to failure. The early history of vacuum switches shows the futility of undertaking an applied research program before the necessary fundamental knowledge is available—or at least seems within the possibility of achievement. One reason “P-Zero” failed was its unfortunate coincidence with other major changes in the location and technique of refrigerator manufacture.

The timing of an applied research project also must be keyed to the pace of technological change in the industry to which it is applicable. Research cannot be characterized as “long range” or “short range” on the basis of the absolute time it is expected to take to go from idea to product. In the fast-moving solid-state electronics business, the tunnel diode went from idea to product to profit in a matter of months; in the more mature lamp business, where further inventions took place and where the problems of life-testing alone are necessarily time-consuming, it was a half-dozen years before Lucalox ceramic appeared in the Lucalox lamp.

In some cases, timing is a minor factor. The discoveries of Man-Made diamonds, etched particle tracks, and new lubricants might have had equal impact if they had occurred several years later—or earlier. In the case of the lubricants, however, their availability at the time of a regular periodic design change for appliance motors was important.

An extremely important element in the conduct of applied research is to create circumstances that ensure the confrontation of scientists with practical problems. Basic to this picture is, of course, the necessity of effective internal communications in the research component and the served organization. The failure of fundamental work to yield practical results, or of applied research to solve the true barrier problems, too often results from the fact that the experimenters themselves are never adequately confronted with the real practical problems that exist. These practical problems can be a stimulating source of fundamental research—as it was for Pasteur, for Langmuir, for Jansky, and many others—just as stimulation can come from the inner development of the “pure” science.

Both sources of research must be watched by the scientists of an industrial laboratory.

To ensure this confrontation it is sometimes advisable to violate the tenets of timeliness described earlier by proceeding to an applied project or a development before it is known or believed that the supporting and contributing knowledge is ripe. The extremely rapid pace of semiconductor science made it reasonable to assume that new scientific knowledge about electron tunneling would be discovered at least fast enough to keep pace with a simultaneous development project on tunnel diode devices themselves. There seems to be no better way to test for unsuspected gaps in the knowledge or ensure the "confrontation" of problems and science than to carry out all the various practical aspects of a technical work—including product development.

In a number of the case histories—for example, diamonds, etched particle tracks, lubrication—success depended on the cooperative efforts of several scientists. One might wonder how it is possible to marshal highly qualified, self-motivated, independent researchers to attack a particular problem in a coordinated and cooperative fashion. Such individuals do not ordinarily welcome interruptions of their own research by an investigation along a different line. Moreover, scientists, especially good ones, may not like to work with others in cooperative investigations—or so it is commonly believed. However, this trait varies from person to person and is independent of how good the scientist is. Many outstanding people are untroubled by whether they share the credit of a successful investigation with others or whether they do it alone. Moreover, the willingness of research scientists to engage in either individual or cooperative investigations is influenced by the climate of the laboratory. We have attempted to attach neither a penalty nor an added reward to a particular set of associations in an investigation. The success of the investigation alone ought to determine the honor and reward of participants in it. Above all, the importance and excitement of the objective of the group effort are a vital requirement for creative cooperative work. For a truly exciting investigation—such as superpressures, or the fundamentals of lubrication or etched particle tracks—there has seldom been difficulty in attracting a group of highly competent, highly motivated, highly creative individuals to the work, particularly if the managers and leaders are themselves enthusiastic and excited about the project. In short, the sociological climate in the organization must encourage cooperative efforts when they are needed.

Beside the factor of climate, there is also that of the flexibility of the industrial research scientists and their natural contact with colleagues. They have, perhaps, a somewhat greater freedom to change their investigations than academic scientists who are responsible for graduate students, whose work cannot be easily changed midway toward a thesis. This

flexibility is an aid in assembling teams of individuals when needed. Moreover, industrial scientists, without the time-consuming responsibilities of courses and graduate students, may tend naturally to interact with each other more often and more closely than do the senior research scientists in an academic environment. Thus, the industrial research scientist has perhaps fewer encumbrances against changing his field of investigation on short notice—and has perhaps more natural interactions with his colleagues—than an academic scientist. These features assist in the formation of cooperative research investigations when they are called for.

Finally, such cooperative research groups could not easily be formed—or disbanded when their purpose has ended—unless there were a reservoir of scientists to draw from or to return to. The kind of talent needed for a new cooperative research opportunity cannot be quickly recruited from outside the organization; a quick response to a new situation in applied research almost always requires staffing the new effort from within. Thus, the parent organization must already have many of the talents needed in the new effort. The problem of converting a research man from one field to quite a different field is also a long and time-consuming process. This is not to say, however, that research investigations are an activity for which one has a standard set of very special skills and talents that can be assembled, like the pieces of an erector set, to meet any requirement. The problem of creating mission-oriented or focused groups relatively quickly can only be solved if there is a broadly based background research effort—or if there are many other focused projects from which people can be drawn. The problem of disbanding focused investigations, a problem frequently encountered in the management of research and development, can be solved by an organization in which new focused investigations continually arise to utilize the people who come from the disbanding investigations—or by having a broad background research effort to which these people can return. A diversified industry, with many research interests, has a definite advantage in this aspect of applied research because numerous, concurrent, focused programs and a continuing body of background research are normal features of its research and development scene.

The serious problems of mission-oriented research and development groups usually arise when these groups are formed in isolation from other related missions and in isolation from broadly based, discipline-oriented research. In such cases, the group has nowhere to turn once the mission has been accomplished.

Another feature of industrial research, related to cooperative efforts, is the long memory of problems and investigations that persist in the organization. For example, memory of the problems of the vacuum switch persisted for decades in the laboratory. This organizational memory is a source of great strength. It tends to persist because it is

shared by a number of individuals. The scientists who have been in a cooperative investigation collectively remember the problems faced, the things learned, and the barriers not successfully overcome; and the loss of one of them will not destroy the memory of the whole organization.

### The Role of Patents

Patents continue to play a basically important role in modern industry. In every investigation it is hoped that discoveries will stimulate inventions. The scientists are asked to disclose all ideas they believe might be patentable. If management decides that a patent application should be filed on a disclosure, the scientist is generally given a modest bonus at the time of filing—a token recognition of an inventor's achievement and of his contribution in framing the patent application within the rigid requirements of the patent laws.

The patents, pending or issued, on inventions originating in research play an important role in the commercial development of the work. To those who are intimately engaged in research and development work, the doubts sometimes expressed about whether or not patents are an important element in stimulating technological change in our economy seem highly absurd from both the commercial and the social points of view. Without adequate patent coverage, operating management may be unwilling to invest in a product-manufacturing enterprise, which may be immediately copied by a competitor, who, of course, has not borne the research expense of the product development. With adequate patent coverage, the entrepreneurial manager is encouraged to take the additional business investment risks in bringing a new product to market, and he will further invest an additional research to extend and expand his product business.

Another basically important role of patents is to permit the prompt publication of scientific results. Without patent protection, industrial scientists would have to rely on secrecy to retain an equity in such results. Broad-scale industrial secrecy would greatly slow down the pace of both scientific and technological progress, since all modern advances are built on prior results. The publication of an experiment on tunneling by a Japanese scientist stimulates an American scientist to invent a tunnel diode—and *ad infinitum*. In the Free World, industrial scientific research is promptly published, and the patent system is the basic enabling act that makes this possible.

### Using Applied Research—The "Technology Transfer"

The critical phase in any research and development sequence is the transfer of results from the research and development organization to an "operation" or client, or using organization. The techniques for making



such transfers are extremely variable. Perhaps the most serious mistake a manager of technical activities can make is to believe that the process is governed by some formula and that adherence to a practice or operating method will accomplish this result. The identification of a uniform pattern for surmounting the "application interface" has thus far not been accomplished.

However, one general statement that can be made is that only in rare cases is it possible to effect this transfer by the simple exchange of "software" between the research organization and the operating component. The writing of reports is certainly not sufficient, nor is the giving of lectures and verbal exchange of information. Almost invariably the transfer of technology requires the demonstration of technical feasibility. Thus one must demonstrate a piece of Lucalox, fashioned in the laboratory into a suitable shape for use in a lamp; or a quantity of diamonds sufficient for making a grinding wheel; or a tiny FM transmitter incorporating tunnel diodes.

The "demonstration of technical feasibility" will not ensure success, however, and there is no substitute for making it possible for research people and manufacturing people to work side by side for a period of time on the new development, either in the laboratory or in the operating component. This can occur through having the early stages of product design, of prototype construction, or of pilot-plant construction take place in the research and development organization, with operating people participating, or it may occur through the actual transfer of research and development people into the operating component.

The transfer activity just described will not ensure the success of the effort, but it includes the principal items that the research organization itself can do in this direction. Beyond what the research organization can do and should do is a host of considerations concerned with the receiving component. The "operating management" needs to consider and deal effectively with its end of the chain of events which started with research and may lead to a new product or business. Thus profitability of the component, availability of technical and managerial capability, compatibility of the new technology with present products, manufacturing capability and markets, and a host of other considerations will finally determine whether a new idea will achieve economic maturity.

We believe that many of the features suggested by the case histories and the lessons we have learned from them—as well as from many other successes and failures in a private industry—are also applicable to the problems of managing the Nation's applied research.

## TECHNOLOGY TRANSFER

by CHARLES KIMBALL

The dissemination and transfer of knowledge about scientific and technological discoveries has been a matter of importance since the inception of organized efforts in science and technology in the early part of this century. By and large, new knowledge in science is dispersed in a relatively straightforward manner, with very few instances in which scientists do not get the information they need quite readily. These dissemination techniques are conventional, and have stood the test of time—scientific papers, scientific meetings, and person-to-person communication.

Similarly, the transfer of knowledge concerning new developments in *technology* is not a new issue. The general process for applying new technology—proceeding from basic research through to initial applications—is well understood and widely employed. The steps involve an understanding of the basic phenomena, a clear definition of the mission, applied, or “mission-oriented” research, and the development of relevant designs, processes, and equipment. Such transfer has been carried on quite effectively for many years, especially within large companies, with many outstanding examples of technological and economic progress resulting from such vertical-transfer activities.

The follow-on process of technology transfer, i.e., communication or coupling between persons who know the new technology and those who need to know, is less well understood.

For some years now, and especially since the advent of very large Federal research and development programs in defense, space, and atomic energy, there has been concern about finding applications in the civilian industrial sector of the economy for the advances in science and technology coming out of the extensive Federal research and development effort. This concern stems from several factors: an apparent distortion or skewing of the national research and development resources; the desire to accelerate the civilian economy through technology; and the need to get the greatest economic benefit from the expenditures of public funds on science and technology.

This paper is concerned principally with the transfer of technology originally developed for defense, space, and atomic energy and its employment in both the public and private sectors, in applications that may

be quite different from the purpose for which the technology had been originally developed in such Federal and mission-oriented programs.

There are many useful examples of such "horizontal" transfer of technology from the Federal sector to private and industrial uses, and to other public needs and purposes. Some technology is being transferred as well between nations, between one region of the country and another, and between one industry and other industries. A listing of major technology transfers from the federal sector to the private sector would range from widely recognized accomplishments to many smaller, less obvious incidents.

Technology developed for radar applications during World War II has been extensively applied to commercial TV circuits, air-traffic control, and weather-warning systems.

Military jet-engine technology has been improved and applied to worldwide use in commercial air transport.

Isotopes developed through the Atomic Energy Commission program have found extensive use in industrial processes, agriculture, and analysis techniques.

The computer industries, initially stimulated by defense and space programs, have developed diverse and significant applications in many other fields.

Glass technology was given considerable new dimensions by the need for lightweight, filament-wound, glass-fiber rocket casings. These techniques are now extensively employed in the manufacture of storage and pressure vessels, including railroad tank cars, for civilian uses.

Inorganic coatings that change color as a function of temperature, coming out of the aerospace programs, are used as temperature indicators.

Some 50 applications for manufacturing licenses have been filed or granted for new inorganic silicate paints developed for space applications. These are far superior in many ways to epoxy paints, and can withstand a temperature of 4000 degrees Fahrenheit.

A magnetic hammer that causes metals to flow readily is employed to smooth out welding distortions in rocket-fuel tanks and in commercial shipbuilding.

Transfers of management technology, including Program Evaluation Review Techniques (PERT) and Critical Path Method (CMP), have now permeated the major construction companies, and were derived from complex Federal activities such as the Polaris Fleet Ballistic Missile Systems.

The Planning-Programming-Budgeting System (PPBS) is another example of transfer: to State and local governments, and to private industries.

These are a few examples of "horizontal" transfer, as defined above. For the purposes of this paper, they are not to be confused with other, older Government programs that have been both creating and transferring technology for some time. The prime example is the extension service of the U.S. Department of Agriculture. The extractive metallurgy techniques of the Bureau of Mines, the National Institutes of Health's medical technology, and the National Bureau of Standards' services to industry, are other instances of this accepted Federal role.

Technical advances and technology transfers, whatever their origin, can be important factors in the socio-economic growth of nations, regions, industries, and communities. They possess not only quantitative dimensions, but qualitative dimensions as well.

The current interest in technology-transfer opportunities is timely because the Nation is experiencing rapidly accelerating technical, social, and economic change. America must make the best possible use of all its technical resources to enhance our national posture and the quality of our lives. The natural process of technology transfer would take a considerable period of time to assimilate, as would making other uses of Government-financed technical advances if the process were allowed to work itself out naturally. Even the relatively straightforward transfer of the jet engine from military to commercial use took more than a decade. More complex transfers have taken longer. It is possible that the volume of research conducted over the past several decades may have overwhelmed the natural process of technology transfer. There is an implied belief that the natural process no longer works well enough, and that formal intensified transfer programs can bring about broader uses of new technology more quickly and at less socio-economic cost.

For these reasons, over the past several years, certain Government agencies have organized and now operate formal technology-transfer programs. The National Aeronautics and Space Administration is the principal current sponsor of such efforts, and the Atomic Energy Commission has begun transfer programs for non-nuclear technology. More recently, the Department of Commerce has become involved through passage of the State Technical Services Act. The object of these efforts is not only to move Government-developed technology to the industrial sector, but to State and local governments as well, where the opportunities for transfer application may be appreciable. For example, the systems-analysis techniques developed for military purposes may have value to law-enforcement agencies.

The experience derived from these overt "horizontal" transfer efforts reveals one basic fact—that there is simply not enough known about the transfer process on which acceleration and emphasis are being directed. All the programs to date have been experimental, but they have

been so in an operational sense. The measures of success cannot be determined just by the number of transfers accomplished per unit of effort.

In fact, technology transfer is perhaps most significant at the level at which cumulative small bits of new information are recombined and put to new uses. At the present time, it is difficult to manage this process and to measure its profitability. Explicit research into the transfer process is needed. This dimension should be added to the existing operational-experimental scope of these agencies' activities and augmented in other ways.

Those working in the transfer field have come to appreciate the extremely complex nature of this transfer process, recognizing that its successful application and acceleration depend upon many factors. These range from the types of persons involved in the process (the primary issue), the definition of the purpose of the transfer, and the increasing economic relevance of these programs. The process of recognizing a technical advance and establishing its significance in other fields, depending on certain adaptations, has led to the observation that no single transfer technique is suitable for technology of such variety and quantity as is being generated today in the Federal sector. (It is essential here to define *new* technology as that which is new to the person who needs it, or will use it, and not necessarily new to the technical world in general.)

### The Climate for Innovation

Five general interrelated conclusions concerning the transfer process can be drawn. The first is that there must be a far better climate, more receptivity to innovation and change, if there is to be more effective transfer. There are, for example, appreciable differences in this respect within regions and communities, and within industries and individual companies.

The report on "Technology Innovation: Its Environment and Management" stated that certain cities, such as Boston and Palo Alto, "have an innovation environment that has led to the generation of many new technical enterprises. Chicago and Philadelphia are almost devoid of this type of new business generation." The technological gap between certain agricultural and industrial states may be greater than that existing between the United States as a whole and any major European country.

Many companies, both large and small, are not adequately receptive to employing new technology generated outside their own walls. Exceptions are found most extensively among larger companies that often have specially designed internal apparatus to bring this about. One also finds many smaller companies with highly knowledgeable management who meet this test. Many companies do not wish to make obsolete either existing products or equipment, recognizing as they do the marketing

and investment risk implicit in new-product development. Perhaps for this reason most transfers have been improvements on existing processes, either improving product quality or reducing production costs. Perhaps more effective transfer and a higher utilization of Government-developed technology would ensue if some additional inducements were provided, such as adequate patent protection and investment tax credits.

### **The Necessity for Problem Definition**

The second general conclusion is that the technology-transfer process must be problem-oriented or need-oriented. Overt transfer efforts could, for instance, be directed at one or more specific socio-economic public needs—such as air and water pollution, waste management, traffic safety, improved mass transportation, crime prevention, and improved education and training methods. Many of these, under the proper circumstances, could gainfully employ technological processes developed with Federal funds for different mission purposes.

One example of a specifically targeted mission-directed effort involves biomedical applications of aerospace technology. Its relative success illustrates the value of identifying a group of users with common technical problems, i.e., physicians and medical researchers in several medical schools who uncover and describe quite specific problems. They work with certain independent research institutes who search the supply of federally-derived technical information to help find solutions for these specific problems. Medical-instrumentation problems recently solved by this effort include a special muscle accelerometer to diagnose neurological disorders, employing principles and devices originally developed for a micro-meteorite detector; a new technique for applying electrocardiogram electrodes to children, originally developed for instrumenting National Aeronautics and Space Administration test pilots; and a modification of the astronaut's helmet for research on oxygen consumption.

### **Research on the Transfer Process**

The third conclusion is that the transfer process is not only extremely complex, but that much greater efforts must be expended to understand it, i.e., how a transfer could or could not take place, and how to design future transfer programs to capitalize on this insight. Not enough is known about the "natural technology-diffusion process" for which the acceleration and intensification seem essential, principally because so very little analytical and empirical work has been done to advance knowledge about the process. It lacks a rigorous analytical framework within which the problem of technology transfer can be appropriately analyzed and understood.

The steps that need investigation (concerning their relative importance and interrelationship) are (1) finding the technology, (2) screening for relevance and emphasis, (3) "packaging" for effective use in user terms, (4) bringing the technology to the attention of relevant users, and (5) demonstrating its social and economic value.

Perhaps the circumstance can be illustrated by a chemical analogy. A chemist studies the activities of natural compounds in some known beneficial use. He then correlates this activity with chemical structure and, in this way, in a highly organized fashion that is capable of extrapolation, the method can then be employed to search for new compounds that might act similarly. The same route must be followed in developing transfer programs to promote rapid and valuable applications of new technology. Only then can operations be designed and instituted that support the positive features of the "natural technology-diffusion process" and reduce many identified barriers. To summarize this point: there is now no single "best way" to intensify technology transfer, nor is a "best way" likely to emerge soon.

Too much emphasis has been placed on the mechanism of transfer. More emphasis now needs to be placed on two other, more important, elements. The first of these is the make-up of the supply (information to be transferred) in terms of quality, quantity, and relevance. More must be known about how the supply source works, and how its content can be modified and distilled to make it more meaningful and, if necessary, develop or evaluate alternative mechanisms to bring this about. Secondly, more needs to be known about what causes an ultimate consumer of transferred technology to seek and, then to accept and to employ, that technology. These two elements, if better understood, will give the potential recipient more confidence in the source.

### Barriers to Transfer

The fourth general conclusion is that there are a large number of "transfer barriers." Some of these will always exist and, perhaps, can only be recognized in the design of future transfer programs. Other barriers, once recognized, can be overcome by designing transfer methods that by-pass or surmount them. The three major barriers are (1) human resistance to change, (2) inadequate skills and narrow viewpoints of people who should be involved in the transfer process, and (3) poor producer-user relationships.

Some of the barriers may not be surmountable under any conditions. One of these, the underlying psychological resistance to change, predominates as a human characteristic at any time and in any period. The two other barriers may be changed only on an evolutionary-time basis, but hopefully more rapidly.

The second and third barriers mentioned have a direct connection with the formal process in our system of higher education. In fact, one could go so far as to say that the entire subject of technology transfer should be dealt with in terms of the "people-transfer process." The problem of information retrieval and transfer of documents is not the main issue. Hundreds of technical reports, sent to a person or organization not capable of handling them, or not interested in their interpretation and reduction to commercial or other mission practice, are useless. Some hope has been expressed in the past that scientists and engineers who are working on Government-financed research and development could be induced to be more concerned with the possible implications of their work for industry, or for other problems of a public nature. This has not been a fruitful source of transfer largely because the type of person whose aspiration is to achieve scientific and technical progress for himself or for his mission is generally not the type who also seeks to effect technology transfers, although there is an encouraging, emerging interest in this matter among some scientists. Technical and management people in the private sector whose sole association has been with Federal research and development programs seldom possess the necessary attributes or viewpoints to develop products or processes for the highly competitive commercial market.

The technology-transfer process is social and economic in form and purpose, rather than scientific or technical. The decisions to use technology, particularly in industry, are economic in nature, rather than technical. Therefore, persons concerned with transfer as their main interest need to be judged on a scale of accomplishment different from that employed to evaluate and reward conventional research and development personnel. The transfer person, for example, must be able to assist the potential user in defining problems and opportunity. Having done so, he must have sufficient technical familiarity to search the supply of new technology to form the basis for a problem solution. This depends, of course, on accessibility and intelligibility of knowledge resulting from the results of "outside" research and development efforts. This same person or persons will then be able to adapt this newly acquired knowledge to specific requirements of the potential user, to whom the new knowledge will be applicable only if it appears to him to be beneficial, in his terms.

To stimulate, accelerate, and promote valid transfer through overt organized programs circumstances must be provided to make it easier for individuals and companies to be innovators: demonstrations that markets exist for new technology products, that the risk is reasonable, that the cost-benefit ratio is favorable, or that the competition in the area is not excessive. The extent to which this is done for different potential users will vary, but the more this is done by the transfer agent,



the easier it will be for the ultimate user to become an innovator in his own right.

The type of person described here might be called an "applier of science" or, preferably, an "applier of technology." He will have two outstanding personal characteristics. He will understand the world in which commercial forces operate, and he will have some broad technical background. It may be advisable to select appropriate generalists and supply them with the necessary technical facts, rather than attempt to convert a conventional scientist through exposure to commercial reality and to the requirements of corporate management in particular. Experience shows that most research scientists and engineers make rather poor transfer agents. Bringing about transfers is not the way they achieve satisfaction. Their education, experience, and general thought pattern do not equip them to deal effectively with the major factors that control transfer. To many persons who generate new knowledge, its transfer to the commercial world or other "practical" purposes is often considered a secondary assignment, one to be concerned with only incidentally or under duress. New technical ideas are transferred and implemented by persons—not by reports—and for persons to do this effectively, they must operate in an environment that is conducive to new-enterprise generation.

A number of research scientists and engineers, after a few years of exposure in industry, turn out to be quite capable entrepreneurs and develop intensive interests over the entire spectrum of subjects concerning a business. These persons often make very good "transfer agents," possessing adequate technical background to appreciate the technical implications, with the added incentive of contributing to business interests and knowledge.

### **The Transfer Agent is the Key**

The fifth and last conclusion is that the time has come now to consider carefully the deliberate development of techniques, university curricula, experiments, and perhaps even new institutions to provide the Nation with many more effective persons who can serve as "appliers of technology." These are not merely persons skilled in the practice of applied research. Rather, they are persons who, when properly selected for motivation, and when skillfully and purposefully educated and trained, will help materially to identify and surmount some of the transfer obstacles.

The technical entrepreneur, the champion of a new idea, is frequently the main force behind technical change. His main strength may be enthusiasm, ingenuity, and a commercial or public purpose, and not a basic-research point of view. He may be more distinguished for these attributes than for his technical expertise. His role is a vital constituent

of transfer and innovation. These individuals need to be described, characterized, and identified as early as possible in their professional lives to provide them with significant and relevant educational opportunities, and then to provide working environments that will make their contributions meaningful, such as in independent research institutes, in technical consulting firms, and in certain selected corporate industrial laboratories that have demonstrated multidisciplinary transfer capabilities.

Significant technology transfer can, and will occur only when the right people, markets, and ideas coincide with usable technology at the right point in time. Technology, *per se*, may be the least important element in the over-all transfer process.

Several other approaches are suggested. Our system of university education could be very helpful in developing techniques for retraining the proven entrepreneur who is five to ten years out of school, with updating in his fields, so that his entrepreneurial ability will be enhanced. A few graduate business schools have employed this approach. This effort should be expanded, especially in engineering schools. Special summer institutes for university personnel, including selected graduate students having such entrepreneurial skills, would be useful—especially if the “faculty” were experienced, industrial people with demonstrated entrepreneurial skills and insights of their own.

Such specific curricula directed to increasing the numbers and quality of “appliers of technology” would be helpful in bringing about a more fruitful rapport between universities and industry. This is now limited by the unwillingness of many university personnel to relate their research or academic programs to economic or industrial needs. As such “appliers of technology” become more numerous, proficient, and widespread in the United States, the geographical separation of universities and industries will become less of a limiting factor.

Independent research institutes, many of which have already demonstrated competence in multidisciplinary research and development, should be considered as vehicles to promote interchange of ideas and mobility of people among Government, industry, and universities, to foster and to put into practice the appropriate transfer concepts.

### Summary

The economic growth of the United States could be enhanced appreciably with more effective use of new federally-developed technology by the private sector of our economy, and by certain elements of the public sector. Not only would economic growth be accelerated, but many social and community problems could be understood better. Technology transfer fosters more widespread distribution of industrial and commercial activity among the various regions in the country.

The natural process of technical change employing conventional techniques—educational, retrieval, dissemination, and the like—is not adequate in timeliness, quantity, or quality to bring about the necessary accelerated rate of transfer. Research on the entire process of understanding, accepting, and employing technical change, is clearly indicated as a consequence of the growing volume and diversity of technology, and the potential and exciting demands for its fruitful future use.

## TECHNOLOGICAL INNOVATION AND ECONOMIC GROWTH

by ROBERT A. CHARPIE

Technological innovation is the driving force behind economic growth in the United States. In this context, "innovation" means that process by which a new idea is successfully translated into economic impact within our society by providing better products and simultaneously creating new jobs in the manufacturing and application of those products. Thus an idea or invention is a necessary but not a sufficient prerequisite for innovation. Only after an invention is put into sufficient use to have an economic effect is it to be termed an innovation.

Innovation takes many forms. For example, the supermarket is a marketing innovation, as are the personal credit cards which many of us use for instant identification and credit at service stations, restaurants, and airline ticket counters throughout the country. Although such innovations have had consequential economic impact, they do not provide an increase in the technical content of the product involved, but merely serve as a novel means of providing a conventional end result. We shall concentrate in this paper on those innovations which flow directly from new technical ideas, inventions, or discoveries and which result in new products or services with essential technical content. These are what we term "technological innovations."

During the past 25 years whole new industries have developed based on technological innovations, such as television, synthetic fibers, jet engines, and computers. Even more recently we have observed the stimulus of technological innovation in the dramatic growth of many successful new enterprises with the attendant creation of thousands of new jobs. Notable examples of these are Texas Instruments, Xerox, and Polaroid. Each of these companies is based on the timely introduction of a technically based innovation (semiconductors, electrostatic copying, and instant photography) into the marketplace in a useful form at a price the market was willing to pay.

Economists estimate that 50 percent to 80 percent of the constant dollar (real) growth in the gross national product is attributable to gains in productivity, nearly all of which flows directly from new technology. For example, the chemical industry is producing twice as much per man-hour now as it was ten years ago. Thus, technological innovation is truly

the cutting edge of the economic growth process and the economic vitality of the Nation is intimately enmeshed with those companies that are successful in the technological innovation process. As surely as the economic strength of the Nation is a proper concern of the Federal Government, so it is required by the national interest for the Federal Government to understand the technological innovation process in civilian industry and to identify those Government actions which either support or restrict this essential innovative growth.

A cursory examination of the growth patterns of other countries reveals some important differences compared to the growth patterns of the United States. The degree of free choice which operates in our society is certainly not present at the individual level in the Iron Curtain countries, yet they have at times enjoyed phenomenal economic growth. In Western Europe an isolated entrepreneur starts his own company to prove the merit of his judgment less often than in the United States, yet rapid economic growth has taken place there too, particularly in the years immediately following World War II. From a few such observations we can draw the tentative conclusion that at different levels of national development economic growth can proceed rapidly by more than one route. I believe, however, that once a nation achieves a highly industrialized and strong economic foundation, further economic growth depends largely on the successful development and use of new technology through the innovation process.

The large sums of money invested in industrial research and development in this Nation result in the identification of a continuing stream of technologically based innovative opportunities that are evolutionary in nature. These provide the steady year-to-year improvements in product quality and productivity gains which sustain and strengthen our economy.

But I want to remind you that technological innovation can be revolutionary as well as evolutionary. Major innovations often occur across conventional industrial lines, rather than being fostered within a given industry. Thus, synthetic fibers were created by the chemical industry rather than by the textile manufacturers. Revolutionary innovations often cause widespread disruptions within the industry which is affected. Yet the resulting price which is paid in terms of turmoil and upset within a given industry is more than compensated for by the broad benefits to society which customarily flow from successful innovation.

Indeed, the textile business, although very different from what it was 30 years ago, became stronger, healthier, and more profitable as a result of the technological disruptions. Instant photography has not stifled the use of conventional photography but instead has stimulated competition in photographic products to the point where photography is now the province of many more consumers than ever before. The jet engine, which profoundly changed the structure of the aircraft manufacturing

business, nevertheless was the spark for the explosive growth of the aerospace industry which has brought worldwide travel to a larger segment of the population than was earlier dreamed possible and has made international boundaries less formidable obstacles to expanding trade. Thus Government interest in encouraging technological innovation in this country must be based on the realization that the ultimate gains to society from successful technological innovations are generally considerably greater than the temporary and painful upsets and dislocations which sometimes come from innovative activity.

Approximately 90 percent of the research and development activity in the private sector of the economy arises from the expenditures of well under four hundred large corporations. Several studies of the innovative process, however, have shown that individuals and very small organizations are responsible for a disproportionately large fraction of the revolutionary and successful large-scale innovations. Thus total research and development expenditures cannot be equated with successful technological innovation within the sponsoring organizations. In order to explore some of the reasons behind this apparent discrepancy, it is necessary to examine the environment and personnel involved in the technological innovation process.

The most important figure in this process is a man whom we shall call the entrepreneur. Every day ideas are stillborn for lack of a commitment to follow the necessarily painful and extremely hazardous path required for any idea to become a successful part of the economic scene. It is the entrepreneur who translates a sound idea into a successful innovation. He is often a rugged individualist, usually technically oriented, who is committed to the value of an idea and the creation of a business based on it. Often he is an employee of a large multi-industry company and is dedicated to seeing his idea pursued to the fullest, whether *within* or *outside* that company. Recent studies indicate that over half of the successful new technological enterprises in this country were started only after the entrepreneur was unable to persuade his original employer to develop and exploit his concept.

A second key factor in the innovation process is the venture capitalist in the form of a wealthy individual, a progressive bank, or a sufficiently dynamic company which is willing to provide sufficient risk capital to appraise the specific opportunity and provide financial backing for its introduction to the marketplace.

The combination of ingredients for success in the innovative enterprise are clear: the inventor-entrepreneur and adequate financing, plus competent management and key skills in the form of employees and/or consultants, together with understanding and encouragement on all fronts. The path followed by a man and an idea joined in a commitment for exploitation is often an unrewarding one. For every successful innovation,

many more starts are doomed to failure. In order to attract the high risk capital necessary to translate the idea into an innovation at the marketplace, the financial rewards of success must be sufficiently great to offset the inevitable losses incurred by many other promising ideas.

Many aspects of the newly-formed technological enterprise have been covered recently by a report of the Panel on Invention and Innovation which was convened by the Secretary of Commerce (1). That report presented a broad study of the small company environment in which innovation has proceeded so often in the United States. I would like to add a new dimension to that study by examining here the problems of technological innovation encountered by those large American corporations which have been consistently successful technological innovators and which are deeply committed to discovering new technology for the benefit of our society.

A company can grow and become more successful in the eyes of the stockholder by pursuing some combination of four courses which are available to it. First, it can improve its profitability by reducing costs. Second, it can expand its penetration in the markets it is already in with the products it already has. Both of these options require a continuing evolutionary improvement in the quality of its products in order to meet ever more intense competition. Third, the company can grow by creating new products to serve its current and immediately accessible markets. Finally, the company can grow by successfully creating completely new businesses based on technology which it creates itself or acquires. The fourth option carries with it the potential for the greatest growth and also the greatest potential for failure.

It is clear that the large, successful, broad-based corporation in America has the same basic problem as does the individual entrepreneur who, even without the backing of a large organization, is deeply committed to a new technically-based idea. That problem, simply stated, is to find a way to translate a given idea into impact in the marketplace thereby providing growth for the company involved in satisfying a specific need expressed by the American market. Although the goal of the large company is the same as that of the individual entrepreneur, its execution is very different. The entrepreneur is irrevocably committed to a specific idea, while the large research-oriented company usually has many alternatives from which to choose.

Time has taught the American technological industry that a well-conceived and executed research program will more than pay for itself over the years by constant exposure to the new surface of the science and technology discovered within that program. "Research" in this context is (1) an expansion of the fund of knowledge conducted in such a way as to identify new commercial opportunities based on technology, and (2) a systematic attempt to find solutions for known problems which

must be solved before the company can proceed with its defined program of action.

Whether such a research program is termed basic or applied has more to do with the approach and interests of the men doing the work or the motivations of those describing it than with either the quality of the work or the specific work assignment. Thus the very large expenditures on research which our major companies make from year to year must be understood as providing largely a window on opportunities as well as the knowledge necessary for the companies to solve the new business appraisal problem which is so difficult for the individual venture capitalist. It is easier for a company armed with a sufficiently large and competent technical organization than it is for any venture capitalist to select the best opportunities for investment from a group of highly-sophisticated, complex and often inadequately defined technically based business concepts.

Today most successful large technical companies do this job through a process known as total venture analysis. In a total venture analysis an attempt is made at the earliest possible time to describe cost, and schedule the total business efforts in which the company would be engaged, including the development of the idea into a final product and its introduction and distribution in the market. This analysis includes estimating the character of the product, its performance, its manufacturing cost and its markets, including both the price the market will bear as a function of time and the penetration which can be achieved by the company. It is necessary to identify what the competition is, both as regards products which are displaced by the new idea and industries which must be invaded in order to exploit the idea to the fullest possible extent. At an early stage it is necessary to estimate not only how much capital will be required for investment in new skills and facilities but to pinpoint a tentative timetable on which those investments must be based.

The time value of money is a dominant consideration in most such total venture analyses. Companies will scrutinize carefully both the size and the timing of the investment required to mobilize and execute a new business venture. This is simply because the money which is used to finance such an undertaking could be used in other ways within the well-understood existing businesses of the average company with reasonably predictable results. Thus it is necessary for each company, based on its own performance history and its own circumstances, to select among alternative business opportunities by taking full account of how the resources could be used in relatively less risky ventures. Once a company has completed a total venture analysis and has decided to launch a serious innovative venture, however, it usually has most of the resources which are required for success.

Such a detailed analysis is made necessary by the very nature of the large technology-based company since a variety of investment alternatives



are available to it as a result of its research program and its great familiarity with its own product technology and specific markets. In order to compare these alternatives, it is necessary to do so on the basis of a current best view of each total venture under consideration. This kind of analysis tends to preselect for continued development two classes of innovative ideas which fortunately complement one another. Class one consists of those innovations to which are assigned a relatively low risk failure, but which by virtue of their low risk and relatively good predictability are usually evolutionary in character and thereby do not represent the dramatic upsets cited earlier for some innovations. The second class of innovative efforts which will remain part of such a program is made up of those ventures for which the risks of failure are relatively high but in which the rewards for success are very great indeed.

Within the large company environment it is possible to provide the special skills related to tax and legal problems, market analysis and development, financial control, product development and manufacturing which are required in every successful new venture. Thus if the idea is sound, both in terms of product concept and the evaluation of market desire, the large company can foster successful technological innovations time after time.

Nevertheless, many large companies have abandoned sound new venture concepts in the critical early stages of the innovative process. Many well-established and successful companies appear unwilling to pursue innovative ideas which they originated because they do not understand the different markets in which the innovation would be expected to compete. Also many companies refuse to be innovative when there is a risk that a new posture may cause some of their well-established customers to turn to competitive sources of products which are already part of the profit-making base of the company. Finally, the very sophistication of these corporations which causes them to examine a new business venture through a total venture analysis (leading to an elaboration of discounted cash flows, appraisals of business alternatives, management priorities, and planning and review boards) is more likely to lead to an early re-evaluation of a given innovative risk with the decision for abandonment than in the case of a small company which has no choice but to pursue innovation since abandonment means bankruptcy. History shows us time and again that it is necessary to have substantial staying power in order to see even the best ideas through to complete success (color television is an example). Thus, although a small company may be more likely to be committed to a revolutionary idea leading to dramatic economic impact, the chance of successful technological innovation is much greater in a large company if and when such a company can become totally committed to such an idea.

It is increasingly important, if we are to continue the sustained growth in per capita gross national product that has characterized this Nation's economy in the 20th century, that we encourage productivity gain through successful technological innovation. In these times our Nation is engaged in a higher level of total business than ever before and a greater fraction of that activity is related to the achievement of established national goals than ever before. There is no question but that our high level of productivity can support a large amount of social innovation as well as technological innovation. We must remember, however, that the resources for successful large-scale social innovations, whether the objective is the practical abolition of poverty, the elimination of air and water pollution, the development of new concepts of urban living and mass transit in this country, or simpler things like highway beautification, must originate from a more rapid upward growth in the real gross national product if we are to successfully solve *all* of these problems in the coming decades.

In the March, 1967 issue of *Fortune*, it was proposed that a real growth rate of 4 percent in the gross national product is the maximum that this Nation is likely to achieve in the next five years, compared to an average growth of 5.4 percent for the past five years. A potential for growth of only 4 percent was based on strong consumer demand in the light of the predicted growth of the labor force, year-to-year gains in productivity, and average hours worked. The only non-inflationary way in which a more rapid economic growth can occur is by large and simultaneous increases in productivity *and* consumer demand.

Productivity increases hinge on technological innovation and make possible an increase in real disposable income proportional to the increase in productivity, if demand is sufficient. Technological innovation for the consumer market is necessary to provide entirely new products satisfying broadened needs, rather than merely to substitute for products already available. Thus innovations are required in order for the economy to sustain the vigorous growth required to provide the resources to support the social programs which this Nation has set as its goals. Under these circumstances, it seems logical to me for the Federal, State, and local governments to become active endorsers, supporters, and encouragers of the technological innovation process wherever it occurs in our society.

The Panel on Invention and Innovation has argued that the Federal Government's role should be to serve as the cohesive force through which understanding can be achieved by all of the important environmental factors which contribute to successful innovation: the entrepreneurs, the venture capitalists, the banks, the universities, and local, regional, and national governmental bodies. These recommendations

of the Panel were aimed at the central problem of achieving the maximum cost-benefit relationship from the expenditure of Federal funds, whether those funds take the form of selective tax code provisions or simply provide the underwriting for a massive educational program. I would like to add to that body of recommendations one additional thought: In considering the total innovative process in the United States, the Federal Government should take the opportunity to encourage in whatever way possible the already successful technology-oriented larger companies to continue to take ever greater risks in the knowledge that therefrom will flow a stream of revolutionary innovation which will make it possible for this society to continue to grow apace of its social aspirations which are so important today.

### References

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## THE EVOLUTION AND PROSPECTS FOR APPLIED PHYSICAL SCIENCE IN THE UNITED STATES

by EDWARD TELLER

Applied science is concern with human problems, seeking solutions in terms of our great and rapidly expanding understanding and control of the physical world in which we live. Less frequently, but no less importantly, applied science consists of basic understanding and associated control techniques in search of applications having human value. Thus, as has been said, applied science consists both of problems looking for solutions and of solutions looking for problems, where the problems originate in our physical, social, and psychological environments, and the solutions derive from our understanding of science.

Most of the massive Government expenditure for research and development is spent in the applied areas of scientific endeavor. This is probably as it should be; in an age in which the applications of science deeply affect our lives in ways hardly predictable from one decade to the next, strong support by the Government is fully justified. This support has helped to generate rapid growth in many branches of applied science. Nevertheless one has to raise the serious question whether this support and our national effort in applied science are properly directed.

One may advance the claim that it was the United States that developed applied science in its modern form. The name of Edison is one of the most appropriate symbols for the quickening of the pace of applied science, which has laid the foundations of our modern industrialized world. The first and perhaps greatest writer of modern science fiction, the Frenchman Jules Verne, hardly ever failed to introduce into his stories an American who in his peculiar way was connected with the rapid advance in the applications of science. In one of his most famous stories, *From the Earth to the Moon*, it was of course a crazy American who was the driving spirit of the lunar voyage. Nevertheless, in reality, it was not the crazy Americans but the crazy Russians who initiated man's exploration of space.

This fact is of the deepest significance, not because of the prestige attached to Sputnik or subsequent feats, but because of its indication that at least some areas of applied science have firmer and stronger support today in the Communist countries than in any other part of the world. At the same time the main effort of the American academic community, to which we as a nation might well look for leadership in all

scientific matters, has been centered on pure science rather than on its applications.

Thus an almost complete reversal of values appears to have occurred in our country. In the beginning of the century, the United States was the undisputed leader in applied science, while our pure scientists were generally not of the first rank. Today the United States has indubitably captured leadership in pure science, a fact that gives real reason for national pride, and even more for pleasure to everyone who can appreciate the intrinsic beauty, the intellectual value, and the probable eventual utility of pure science. At the same time, the interest of our country's academic leadership seems to have turned away from applied science. This has happened at a time when our future comforts and well-being, our safety, and the future shape of our civilization itself are determined to an increasing extent by the myriad applications of science.

I will not claim in any way that our interest in basic science is greater than it should be. I do believe, however, that a more appropriate equilibrium should and could be established between the basic and applied branches of science. How this is to be done in detail cannot be answered in any simple terms, nor can definite conclusions be reached on the basis of abstract considerations. Even less is it possible to evaluate our applied science posture on the basis of statistical information; the numbers relating to such a study are easily comprehended and may give rise to confidence that the situation is quantitatively assessed, but the often unstated assumptions, simplifications, and value judgments cast serious doubt on any conclusions. In particular, attempts to employ such modern and seemingly absolute evaluation techniques as cost effectiveness will be at least as detrimental to progress in applied science as they are elsewhere. A far-ranging discussion in which ample use is made of examples is probably the best way in which progress in applied science can be represented and the best possible basis upon which improvements may be suggested. A series of questions posed by the National Academy of Sciences constitutes an admirable framework for such detailed considerations, and will be used to orient and direct the following discussion.

1. *Given a broad national goal such as defense, health, prevention of pollution, environmental forecasting, stimulation of civilian technology, etc., what criteria can or should be used in arriving at a proper balance of support along the spectrum from basic research through applied research to development in support of each goal?*

Research in applied science is expensive. Appropriations for specific projects must be continually justified, and the consequent budgetary control has a decisive influence on the progress of research itself. Therefore decisions made on a high level concerning goals are of great importance, and the methods of decision-making deserve careful discussion. It

is my impression that these decisions are generally not made in an appropriate manner. In particular, it seems that undue emphasis is given to the desirability of a goal and insufficient attention is paid to questions of its scientific and technical feasibility, and proper timing of the effort. In the following, I shall attempt to point out some deficiencies in the present modes of action. I shall also attempt to suggest improvements, though it should be admitted that, due to the complexity of the problems, real progress in the art of decision-making will have to evolve in a gradual and, in many respects, in a tentative manner.

The consequences of decision-making based exclusively on desirability may be illustrated by the sad story of nuclear-propelled manned aircraft. It is obvious that the weight of the chemical fuel is a considerable handicap whenever aircraft is to be used on any long mission. As soon as the feasibility of harnessing energy from nuclear fission was established, the apparently attractive possibility of using almost weightless nuclear fuel for aircraft propulsion suggested itself.

The project soon ran into difficulties involving the weight of the nuclear reactor. The weight of the radiation shielding was the decisive factor. Use of the greatest ingenuity in designing the best shielding led no further than to the conclusion that the weight of the shielding could not be reduced below a very high minimum. The upshot was that no nuclear-propelled aircraft could be competitive with conventionally propelled ones unless the aircraft was exceedingly large, weighing almost a million pounds (in contrast to present airliner weights of a few hundred thousand pounds). For lighter aircraft the irreducible weight of the nuclear equipment overbalanced the advantage of exceedingly light fuel.

Even for extremely big and heavy airplanes, nuclear propulsion suffered from a most serious hazard. If such an aircraft crashed in the vicinity of a populated area, radioactivity might be liberated from the smashed nuclear reactor, in amounts comparable to those that could be expected from a hydrogen bomb explosion; the resultant casualties could become completely intolerable. This consideration alone should have been enough to eliminate plans for nuclear-powered aircraft or at least to limit such plans to times and places giving complete protection to our population centers. In spite of these weighty arguments, which could have been foreseen, the project was initiated in the late 1940's. More than a decade and almost a billion dollars later, it was canceled with little to show for the intervening effort.<sup>1</sup>

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<sup>1</sup> Quite a few good men were involved in the effort and significant progress in nuclear technology was made. I should estimate that in a well-planned project of this type the results actually obtained could have been produced for not more than 10 percent of the total funds spent. Parts of the project connected with the operation of nuclear-powered planes must be considered a total loss.

A somewhat similar situation arose in the early 1950's in connection with controlled thermonuclear fusion, the harnessing of the energy of the hydrogen bomb, but the results were quite different. Due to the fact that, in spite of pessimistic predictions by the majority of the cognizant scientists, uncontrolled thermonuclear reactions (i.e., hydrogen bomb explosions) could be executed with surprising ease, the question arose as to whether immediate plans should be laid for the construction of controlled-fusion apparatus on a large scale. In this case, however, the laboratories that were expected to participate in the effort were consulted in a careful manner by those charged with making the basic decisions. From this discussion there emerged the conviction that controlled release of fusion energy depends on the solution of problems related to the confinement of the fusion fuel (i.e., hydrogen plasmas) by magnetic fields. These problems are quite different in nature and more difficult to solve than those encountered in the explosive energy release. As a result, a number of small-scale, parallel efforts were started, the total cost of which was on a more moderate scale than those required for a full-scale developmental effort.

Today, 15 years after these events, the original doubts appear to be fully justified. At the same time, due to the experimentation carried out in the various laboratories, considerable progress has been made in the understanding of many astrophysical phenomena involving plasma-coupling mechanisms similar to those encountered in the controlled-fusion process; study of the same confinement mechanisms has resulted in quite promising shorter-range plans for magnetohydrodynamic generators, which may result in more efficient production of electricity from both conventional and nuclear-fission fuels; last, and most importantly, steady progress has been made on an international scale toward the controlled release of fusion energy, and the eventual success, though not yet assured, looks more hopeful than it did a year ago. In the general field of controlled fusion the United States plays a highly respected, though not a dominant, role. It is one case where an imaginative and properly thought-out start has been made, and where a somewhat increased effort might be justified after careful evaluation on the progress of the work to date.

The problem of desalinating sea water is perhaps one of the oldest and most popular projects in the field of applied science. The desirability of such a technological development is recognized around the globe. The economical feasibility did not appear bright for many years. It seems to me that in this case balanced research has been sustained, though there may have been some errors on the side of unjustified enthusiasm for imperfect schemes. Complete success continues to be somewhat in doubt, though recent developments in large-scale nuclear-reactor technology give grounds for increasing confidence that fresh water may be obtained

from the oceans at a fairly high price (perhaps \$50 per acre-foot, or 15¢ per 1,000 gallons) which nevertheless can be economically justified in many parts of the world. The steady improvement in reactor technology during the last few years has been the reason for the present more hopeful prospect. This is a case where the importance of proper timing of the effort is particularly clear.

The most striking and successful modern development in applied science was not based on any clear-cut or conscious requirement. What mattered was the recognition of the timeliness of a project. I refer to the great revolution that has taken place in electronic computers and automata. The revolution began approximately in 1940 at the International Business Machines Corporation, which was then pursuing the relatively modest aim of introducing automation into business accounting procedures.

The effort became more general, not due to massive Government intervention, but rather to the confluence of an old idea and a new technology. The old idea was due to Charles Babbage, who more than a century earlier tried but failed to construct a computing machine. The new technology was the use of electronics, which made the employment of stored instruction programs in rapidly operating computing machines a practical possibility. Among the several attempts made in this area, the collaboration of an exceptionally gifted individual, John von Neumann, and the IBM Corporation proved decisive. Public funds were involved in significant amounts only at a time when the immense fruitfulness of the field had been demonstrated beyond a shadow of a doubt. Even at present this splendid, continuing effort is mostly in the hands of competing private companies, and it is a remarkable fact that in this important field (in which incidentally secrecy never has been imposed) the United States retains unquestioned leadership.

In contrast to the above examples, which illustrate the importance of feasibility and timing as compared to the desirability of a project, one should mention at least two cases in which requirements are so urgent that they should be given overriding consideration. These are environmental pollution and national defense. In both of these instances there can be no question concerning the need for immediate and sustained action. Question, however, must be raised about the magnitude of the required work and the general direction that is to be given to the undertaking.

Environmental pollution, particularly air pollution, is an old concomitant of our industrial civilization. In some cases, as for instance in Pittsburgh, great progress has been made in cleaning up the air. In other instances, of which Los Angeles is the best-known example, rapid increases in man-made pollutants have brought about a situation which is becoming increasingly critical, in spite of determined and intelligent efforts to combat pollution. It remains an open question to what extent



such situations can be improved by enforcing regulations based on present understanding of the problems and associated control techniques, and to what extent further work in applied science is needed to bring sufficient relief. The problem of water pollution is basically similar. In this case a partial solution may be obtained through biological research on the interaction of pollutants with micro-organisms, which may significantly supplement presently available control techniques.

National defense is certainly one of the most vital questions that our generation faces, both on account of the obviously increasing dangers of inadequate defenses, and because crucial changes in defense technology follow each other with increasing rapidity.

The paramount importance of the security of our Nation is recognized by all of us. But from this starting point recommendations concerning appropriate practical action take divergent courses. Answers on an objective or agreed basis are not in sight. For example, one encounters diametrically opposed arguments concerning the question whether more deployment of existing defense systems and more research in the defense area is appropriate, or whether, by contrast, further technical development is undesirable because it may interfere with the creation of international agreements needed to stabilize the geopolitical situation. We meet here one of the most obvious examples in which questions affecting technology in a most decisive way cannot possibly be settled by discussions limited to technical subjects.

The above examples, very briefly stated, illustrate some of the almost limitless complexities that arise in connection with the funding of projects in the various branches of applied science. It is obvious that an improvement of the present imperfect decision-making process cannot be suggested along any simple lines. I do believe, though, that it may be helpful to suggest two general principles.

The first is that the all-important value judgments concerning the results and the desirability of applied science projects must be made by the representatives of our people on a political level; this holds for the space effort, for defense, and for all other endeavors designed to improve our material comfort, our health, and our safety. That this responsibility can be exercised at this level depends on the circumstance that technical facts can be successfully evaluated (though admittedly with occasional difficulty) by people whose education lies in non-technical or in semi-technical fields. This has been demonstrated, for instance, by the excellent staff of the Joint Congressional Committee on Atomic Energy. Such staff work, however, can never relieve the actual decision-makers of their basic responsibility. This responsibility will be fully met only if the proponents of all the various lines of procedure are given full opportunity to express their points of view, and if the representatives of our people go to considerable trouble in finding out about the principal aspects of the

technical situation. In my opinion, it is as much a mistake to believe that these technical points are too complex for a man who has little specific education in science, as it is to underestimate the effort required to develop them to the level on which the political decisions may be wisely made.

The other recommendation complements the first one. It is that the initiative of technological development should not come from above, but from below. The men who can best assess the feasibility and timeliness of a project are the individual scientists whose judgment and vision can lead to fruitful proposals. In many cases, the suggestions may come from individuals and, in others, from the consensus developed in an active laboratory. One can expect that these suggestions will be properly criticized, and that the really hopeful plans will be selected after discussion engendered by the very fact that many sources exist from which such suggestions can and should come.

One important method by which the decision-making might proceed is to start funding on an intermediate level. The funding of new, hopeful proposals may range from hundreds of thousands to tens of millions of dollars; the resulting thorough but preliminary investigations should almost invariably precede massive expenditures, which can lead to the spending of a considerable fraction of our total research and development budget. There should be ample criticism and frequent review of progress in this stage, so that false starts can be terminated before expenditures on projects with little realistic hope of success become excessive. It should be emphasized that research has always proceeded according to the principle of trial and error, and if you want to be certain of committing no error in these regards, you will equally surely preclude all progress.

2. *How can we arrive at a judgment as to what applied research should be supported for general development of the state of the art without being tied to specific projects and their associated time schedules, and what applied research will be more effective if tied to specific projects and fitted into a highly structured plan?*

Applied science is in fact the bridge between pure science and engineering. The question posed relates to the two ends of this bridge. On the one side general development of the state of the art is usually quite close to pure science. On the other side specific projects fitted into a highly structured plan are the immediate preamble to routine engineering.

The early and usually more modest phases of applied science are as a rule connected with the general development of the state of the art. This phase can and should be encouraged at several laboratories. The specific projects fitted into a highly structured plan may become quite expensive and may therefore have to be limited to a smaller number of laboratories. It seems, however, that in case of important projects there is a real ad-

vantage in maintaining at least two parallel efforts in order to obtain the stimulating effects of competition. Increased costs due to some (but not total!) duplication of effort must be borne in order to reduce the probability that a mistaken decision or inappropriate line of development at a single institution may have lasting ill effects.

It might be instructive to consider a few specific examples. The new and vigorously developing technology of lasers is being pursued in a considerable number of laboratories. Some of this work has reinvigorated optics, lending new strength to a branch of physics that is traditionally associated with pure research. At the same time many applications have emerged which range from medical uses in eye operations, through holography, which can store and reproduce a three-dimensional image with the use of a thin film, to new methods of massive energy transfer. It is to be expected that many applications of these researches will develop during the next few years into specific projects of great importance and of sizable proportions.

In the previous section it was mentioned that the Sherwood Project, that is, the development of controlled thermonuclear fusion, has started out as a parallel undertaking in several laboratories. In the past 15 years the number of independent efforts has increased to a noticeable extent. In case the difficulties connected with containment of hot thermonuclear plasmas can be solved, or in the event that a small-scale model of a thermonuclear reactor is constructed, which constitutes an extremely strong suggestion that a solution is at hand, the time may have come to concentrate the main effort in fewer laboratories. Some of the more optimistic workers in this area believe that such a decision will have to be made in the near future. Even if this should be the case, it would be wiser to maintain strong support in at least two laboratories rather than in only one.

One interesting example of an attempt to concentrate development efforts of an advanced character into a single laboratory was an administrative decision made in the early postwar period to terminate nuclear-reactor development work in the Oak Ridge National Laboratory. The decision had a disruptive effect on reactor development, and did not serve the intended purpose of strengthening the Argonne National Laboratory. Fortunately the steadfast determination of some of the scientists at Oak Ridge resulted in the continuation of the nuclear-reactor program at that laboratory. After the passage of two decades, few will question the usefulness of having had two strong competing laboratories engaged in the early effort to transform the wartime nuclear-reactor technology into peacetime applications adapted to the demands of science and industry.

In this connection it might be noted that the antimonopoly laws of our country have brought about a situation in which the strongest and

most successful developer of digital computing systems, the IBM Corporation, is not the only place where significant progress in such computing systems is achieved; a relatively small company like the Control Data Corporation contributes in an effective and beneficial way. It might be appropriate to demand that our Government, which very correctly enforces some duplication of effort in private industry, should more uniformly apply similar procedures in those branches of research and development that are concentrated in Government laboratories.

In selecting the laboratory or the laboratories in which the big effort of development should take place, one should place the greatest weight on technical excellence in the specific field under consideration. It is quite unjustifiable, in terms of both time and money required to attain the goal, to give a project to a less able competitor in order to enhance geographic balance in the national research effort. On the other hand, the developmental phase perhaps should not be carried out in the originating laboratory, if that laboratory is more oriented toward early research than toward development.

Returning to the main import of the question, it is of course impossible to make a selection based on general principles as to the timeliness of commencing either general development of an area or initiating a specific project in a field. This, as most other important questions, must be the subject of a decision based on knowledge of details. The most critical of these decisions is the one that concerns the massive investment needed in connection with initiating a specific project. How difficult such a decision is might be illustrated by one example from the history of wartime development of atomic explosives.

Eugene Wigner is, in my opinion, not only one of our country's most excellent scientists, but is also most unusually adept at evaluating the possibilities and difficulties in an applied science program. He and Enrico Fermi were the outstanding figures in the development of nuclear reactors. At the time when the decision had to be made during the war as to whether a separate laboratory should be formed at Los Alamos for the purpose of developing nuclear explosives, Wigner argued against such a laboratory; he claimed that, once the fissionable material was available, it would be a relatively trivial task to produce nuclear explosives.

In fact, there was some validity to Wigner's argument, and the difficulty of the actual construction of nuclear explosives has been unduly exaggerated. (The real difficulty lies in the production of fissionable materials like  $U^{235}$  or plutonium.) Nevertheless, looking back on the work of almost a quarter of a century, it cannot be denied that a separate laboratory was necessary. No one, neither Wigner nor the proponents of this laboratory, foresaw even a fraction of the problems that would arise in the course of the development. I am relating this example,

specifically mentioning Wigner, because he is the one man whose judgment in a question concerning applied science I would seek if I were to rely on the opinion of any single person.

The lesson, in my opinion, is that in these weighty decisions nothing can take the place of a thorough discussion between the best people available in the field. In the end the decision will have to be made on a high level, and I would suggest to those making such decisions that one should proceed with a specific project provided that any of the qualified proponents can make a good case for it on the basis of need and of technical feasibility in the face of the criticism of his peers. It is rare for a consensus among experts to develop overnight regarding the feasibility or desirability of a project; some will almost inevitably see the light of promise considerably before the rest. It therefore is most inappropriate to require unanimity among the experts before proceeding and the objections of no single man, nor the opinion of a majority of experts, should suffice to stop a development if a strong and well-informed argument has been advanced showing both feasibility and a prospect of real increase in man's control or expansion of his environment.

3. *What is the best kind of training for the applied scientist, and what kinds of research support are particularly relevant to the training of applied scientists in universities? What kinds of training and experience produce the most effective teachers of applied science or engineering? How can Federal support of research and training be channeled in such a way as to encourage the right kinds of faculty development in the applied sciences (engineering, medicine, agriculture, scientific management)?*

The importance of this question becomes apparent when one realizes that most of our massive public expenditure for research and development is in the applications of science, whereas much of the support given to education in physical sciences helps to bring young people into pure science. The result is that our national applied science efforts are, in general, woefully undersupplied with brains, and consequently are relatively wasteful of both time and money. The proper response to this problem is to increase the intellectual level in the field of applied science, rather than to decrease its level of support. In this situation only a determined educational effort can help. The question of how to initiate such an educational effort in an effective manner is of the greatest possible importance to applied science and to the whole problem of how to attain most readily any national goal having a technological aspect.

The real difference between an applied scientist and a pure scientist becomes fully apparent only on the level of graduate studies. On the undergraduate level, there are common requirements in the education of any physical scientist, and these requirements specifically include thor-

oughness and a reasonable amount of breadth, though the breadth of training for maximum effectiveness in applied science may well be considerably greater than that needed in pure science. Therefore any excellent undergraduate school can serve as a good training ground for applied scientists. Nevertheless there are some institutions in which students are better prepared than in others for future careers in applied science. Two outstanding examples of such schools are the Massachusetts Institute of Technology in Cambridge and the Harvey Mudd College in Claremont, California.

Two circumstances have helped to maintain excellence in applied-science education at all levels at MIT—the traditions of the school, and the links that are maintained between novel and imaginative scientific enterprises in the Boston area and MIT. Despite these factors, which have established the best atmosphere in our country for the development of applied science (or perhaps, in a much more subtle way, because of them), there is a strong tendency at MIT to emphasize pure science as well as applied science. In fact, I believe that MIT should be properly called a university, rather than an institute of technology, and that it is a university with a most admirable balance in the pure and applied sciences.

The other example, the Harvey Mudd College, is a small, recently created undergraduate institution. It specializes in education in the physical sciences and gives the undergraduate an early chance for work on individual experimental projects. This gives the student a good opportunity to find out where his real interests, and therefore his real capabilities, lie.

The decisive period in the education of an applied scientist, however, is his work on the graduate level. Here two factors are of particularly great importance. One is the avoidance of too early specialization; the other is contact with outstanding men whose work is in applied science.

Because of the very nature of applied science, its practitioners are very frequently involved in team efforts. This is particularly true when an applied science development has reached the stage of a project. The practical execution requires skills of widely different kinds, as is obvious from consideration of examples, such as exploitation of atomic energy and the exploration of space. The team effort cannot be closely directed from above with full effectiveness; the best team is one that has been self-organizing because its various members could effectively and intimately communicate with each other. It therefore seems to me that in applied science there is a great need for people who can communicate with workers who are experts in fields different from their own. It follows that, in assessing the progress and effectiveness of a university or institute of technology in which emphasis is being placed upon applied science, one should persistently raise the question whether sufficient atten-

tion is being given to broad education in the relevant fields, not only on the undergraduate level but also to a considerable extent on the graduate level.

The second and most essential requirement is to establish direct contact between the student and the leaders in the applied science effort. All students, and particularly the best students, are acutely sensitive to the real accomplishments of their teachers. Unless the example of the teacher sets a high standard of performance in applied science, few students will develop on their own the motivation to excellence and the understanding of the possibilities for intellectual achievement in applied science. One cannot expect that youngsters in their early twenties should discover and appreciate the real challenges in applied science unless they have models in their teachers.

Compared to the numbers of outstanding basic scientists, our academic institutions have few excellent people in applied science. In fact, during the past decades increasing emphasis was placed on basic science in these institutions. Though I am personally delighted to see that in our country basic science has made such great strides, it is regrettable that this has led to such a complete reversal of the situation prevailing at the beginning of the century, when we were excellent in applied science and not outstanding in pure science. Today the opposite situation prevails, at least as far as our institutions of higher learning are concerned. This will lead unavoidably to a deterioration of applied science unless corrective action is taken, since our universities provide higher education and determine the intellectual climate of the next generation.

Fortunately the United States has a number of excellent applied science laboratories. These are partly private industrial laboratories, and partly Government institutions; one may mention as examples the Bell Telephone Laboratories, the IBM Laboratories, and perhaps the laboratories of the U.S. Atomic Energy Commission. In these and other places, excellent applied scientists are available, but too few of them have direct contact with students; quite a number of these outstanding people could and should be asked to help in the educational process.

It is therefore most strongly recommended that close links be established or, where appropriate, strengthened between outstanding applied science laboratories and the appropriate academic institutions. Thus it would become possible for students from academic institutions to direct their graduate programs toward applied science under the leadership of the best men in the field. When one considers the future of technology in the United States, the most important question is how to improve education in applied science. The answer, I believe, consists in establishing proper cooperation between our universities and our applied science laboratories.

It should be emphasized that applied science has many facets. It is therefore suggested that strong educational programs be developed at various applied science laboratories, emphasizing that particular branch or branches of applied science in which outstanding accomplishments have already been demonstrated.

In addition to the applied scientists who are now doing excellent work in their respective fields and who should be given an increased opportunity to participate in the teaching effort, one should also look to the replenishment of teaching faculties in applied science. The programs that are to be strengthened in some cases and newly established in others should educate young people to participate in the work of our Nation's applied science laboratories, and also to serve as teachers in the types of institutions in which they have obtained their degrees. It seems, however, to be of some advantage if a teacher in applied science has in virtually every case a few years' experience as an active worker in an applied science laboratory.

Federal support of these needed efforts can be made available in several ways. Our large national research and development organizations, such as the National Aeronautics and Space Administration and the Atomic Energy Commission, should be encouraged to establish and strongly support these joint educational ventures. Private industry should be given incentives to go ahead with similar undertakings. It is of particularly great importance that the quality of all participating applied science laboratories should be subject to continuing critical scrutiny. Incentives toward participation in the educational effort should be offered only in cases of demonstrated excellence, and, when offered, they should be generous. I am convinced that inducements from above will be solidly augmented from below by the excellent applied scientists in the better laboratories, who have as strong a drive toward teaching as their peers in the pure sciences in the universities.

Another means of support may be established through the National Science Foundation, which, among its current roles, acts as the principal Federal instrument supporting education in the physical sciences at the university level, a role it performs most effectively. It must be strongly emphasized in this connection that our national deficiency lies primarily in the educational aspects of applied science, and not in the Federal support of applied science projects. Therefore one should not divert National Science Foundation support toward further independent applied science projects, except if such a project is in direct support of an educational effort. This is consistent with the suggestion that such educational support should not be on a massive scale characteristic of some of our great national projects. The National Science Foundation could be associated in a most effective manner with our applied science developments through



helping to meet the urgent requirement for a better educational base in applied science.

In planning our educational effort, it is important to avoid curricula that are narrowly directed toward one very specific enterprise. It would seem undesirable for an educational institution to develop into a supplier that sends almost all its students to one laboratory. While specialization toward one area of endeavor of applied science is perhaps necessary in that phase of education that is connected with the Ph. D. dissertation, sufficient breadth must be retained so that students will subsequently be both capable and motivated to make contributions in fields other than their own necessarily narrow specialties. The fact that each school is likely to develop a style and a set of interests of its own can and should be counteracted by ample measures of mutual exchange of personnel and other means of intellectual cross-fertilization. Both the National Science Foundation and other Government agencies should pay particular attention to the creation of an educational system that serves the applied science community as a whole, rather than any one branch of that community.

4. *Is the United States neglecting applied science as compared with both basic science and engineering development? What steps as a nation should we take to attract better minds into applied science? What criteria can the layman use to appraise our national performance in applied science?*

Unnoticed by most of our fellow citizens, a deep change has taken place in the scale of scientific values within the United States. At the beginning of the century, as has been mentioned earlier, applied science was very highly respected, and basic science was often underestimated as impractical speculation. Today in our institutions of learning the opposite evaluation prevails. Pure science is considered most satisfying to the individual and possessing the highest intrinsic cultural value among the varieties of scientific endeavor. This attitude can be supported by strong and valid arguments.

Unfortunately the pendulum seems to have swung too far. Together with the greater appreciation of pure science, a tendency to despise applications of science has been introduced. These are considered below the dignity of a real scientist; they are scorned as intellectually irrelevant and as usually having merely economic interest. Sometimes applications in general are even considered as dangerous to the safety, happiness, or the continued existence of the human race. The example that forces itself on one's mind is the very prevalent wish that nuclear engineering with its awesome consequences should never have been developed, and the fear that more discoveries of similarly dangerous potentialities might lay ahead of us. Remarkably enough, many of the objections by the academic

community to applications of science seem to be limited to the fields of physical science. In the fields of political science, economics, and sociology, division between pure and practical endeavors are not as sharp, and it is considered quite all right to apply the little knowledge we happen to possess. Applied biological sciences, such as medicine, presently enjoy considerable approval, even though developments in these fields, which seem bound to culminate before the end of this century, will surely rival those in applied nuclear science in profundity of effects, both good and ill, on our society. The use of applied genetics and molecular biology to modify the genetic structure and physiology of our children will be more important and also more disturbing than any other single consequence of the scientific revolution. It is my deep conviction that all such applications must proceed, but with a full realization of concomitantly increasing responsibilities and necessary measures to prevent misuses of new powers.

That the strong current in academic circles against applied physical science has not become more obvious to the man in the street is due to the fact that applied science continues to be strong in the United States for two different reasons. One is that many excellent applied scientists working today have been educated before World War II or immediately thereafter, when the present trend was less pronounced. The other is the realization of our political leaders that many goals of applied science are of real importance to national well-being. As a consequence, great amounts of money are spent on applied science developments, but insufficient expenditures are directed toward providing replacements for older applied scientists. As a nation, we are living to a large extent on our reserves of talent in applied science; this cannot long continue.

An interesting example of public and political attitudes can be found in the history of the U.S. space program, which in fact is by far the most expensive single project ever to have been undertaken by our country, in applied science or otherwise. This project has been generally opposed by the rank and file of pure scientists. It was adopted by President Kennedy principally for non-technical reasons. I do not wish to argue in any way that the decision was inappropriate. It may in fact be that the great amounts of money spent on our space effort will turn out to be an excellent long-term investment because of knowledge of completely unsuspected varieties that the space effort will almost inevitably yield, because of the widespread public interest in science and technology it has generated, and particularly because of the impetus and inspiration it has provided our youngsters in scientific directions; all these are badly needed if our way of life is to survive and prosper in this age of science and technology. I therefore do not argue that five billion dollars per annum is too much or too little to spend on the exploration of space. I merely wish to point out that applied science expenditures are in this important case

supported by most of our political leaders and opposed by the majority of pure scientists. At the same time, it is abundantly clear in connection with the space program that the limited interest of the best intellectual talent in this program has produced less than optimal utilization of the great amounts of money spent; this is certainly one well-known case in which we as a nation have paid dearly for our recent apathy toward applied science.

Within a few years the consequences of the present attitudes are apt to have even more serious effects. The continuing lack of a sufficient number of young applied scientists with fresh ideas and outlooks is likely to damage the quality of research in applied science in the United States, and it seems probable that the United States will forfeit its position of leadership in applied science. Therefore the question raised concerning methods of attracting talented young people into applied science is of great importance, not only to applied science but also to the Nation.

It is not easy to explain in detail what has caused the general movement away from applied science and toward pure science in our intellectual community; it is perhaps even more difficult to find methods by which this trend can be sufficiently modified. It is indeed important to maintain our drive toward excellence in pure science; at the same time, we should encourage a fraction of our brightest youngsters to consider applied science as their life's work, not as an act of sacrifice in the national interest, but because excellence in applied science is intellectually as demanding and as rewarding as excellence in pure science.

One of the most obvious and effective procedures has been recommended in a preceding section. It consists simply in establishing effective contacts between students and the leaders in applied science; the intellectual rapport that will develop in many such contacts will provide strong motivation toward applied science careers. Another important technique would be for instructors in basic undergraduate courses to point out the implications of new knowledge for applied, as well as basic, science.

There are additional effective ways to attract young people into applied science. Some of these have been carried into practice on a small scale by a new foundation, The Fannie and John Hertz Foundation. One of these is the establishment of prestigious, well-paying fellowships to support graduate education in applied physical science; the Foundation awards a dozen or more fellowships each year. These are given for three years in case of satisfactory performance and can be extended for even longer periods. The Foundation pays educational expenses plus \$5,000 per annum of personal support for an 11-month year for unmarried students, and \$6,000 for married students. The students are selected on the basis of their academic performance, including grades and faculty recommendations, supplemented in every case by an interview in depth.

In the selection, thorough attention is given to the question of whether the student is likely to devote himself in the long run to a branch of applied science. It is considered important to have the level of personal support quite high, relative to that in the pure sciences, since stronger economic inducements are brought to bear on the young applied scientist to forsake part or all of graduate education in favor of earlier career commencement. This is probably quite damaging in the long term to his productivity, and such inducements must be effectively countered.

A second program pursued by The Fannie and John Hertz Foundation is the establishment of a prize for a young man, preferably under 35 years of age, in a field of applied science. The award consists of a bronze medallion and \$20,000 and will probably be awarded only every other year. The first award was made on April 30, 1966 to Drs. Theodore H. Maiman and Ali Javan for their pioneering work in the development of lasers. It is perhaps noteworthy that both of these men received their original training in pure research and were subsequently attracted into applied areas. One might well ask how often such intellectual mutations will occur in the present climate in our country.

A small foundation, of course, can produce only limited results. But it may also provide a useful example and establish a worthwhile pattern. It appears that these two activities—the granting of prestigious, well-paying fellowships to carefully selected individuals in the applied sciences, and the awarding of significant honors to outstanding applied scientists—will indeed stimulate the interest of young scientists whose inclinations draw them toward applied fields. It is particularly important to direct our attention toward the next generation, because it is precisely among these younger people that the lack of appropriate emphasis on applied science will produce ultimately catastrophic deficiencies. I would suggest that the Congress would be well-advised to authorize the National Science Foundation to initiate programs similar to those of The Fannie and John Hertz Foundation, but, of course, on far larger scales.

Agencies that can usefully participate in these efforts include the National Aeronautics and Space Administration and the Atomic Energy Commission as well as the National Science Foundation. Furthermore, it is important to our national security that our military services place increased emphasis on offering excellent opportunities in applied science to some of their most gifted officers. The future success of each service depends most sensitively on technological developments. These developments are decided today almost exclusively in the civilian office of the Director of Defense Research and Engineering. While this office has made some good contributions in the past, and is making splendid contributions at the present time, the officers in our services have to a great extent lost any determining role in the technical decisions on which the very future of their service depends. This imbalance should be corrected,

and this can indeed be done if the proper opportunities are offered to able young officers to complete their education in applied science. An obvious requirement for success of such a program is that these officer-scientists should be given the same opportunities for career advancement as officers in other branches of the same service.

There is no simple criterion by which our national performance in applied science may be appraised. In fact, it might be even more correct to say that there is no such criterion, be it simple or complex. One qualitative indication, however, could be derived from comparison with accomplishments abroad. Indeed, this was done when Sputnik I forcefully drew attention to the fact that the United States had no natural monopoly on important developments in applied science.

An equally important but much less obvious indication may be found in the comparison of progress in the field of nuclear energy. Here many of the accomplishments are kept secret, and only persons in high political office have access to the data—incomplete as they are—for comparison. Considering the somewhat erratic behavior of our press, it is not to be expected that the man in the street will always or even often get a balanced picture of our national performance in applied science. The national leaders on whose shoulders we have placed some of the greatest responsibilities in these respects can, however, obtain and assess much of the appropriate information through their own efforts, which in turn may be augmented by staffs of able assistants.

5. *In industrial or Federal laboratories, how can we appraise the effectiveness of the laboratory in the performance of its missions? How can the laboratory audit its own performance, and what criteria can the management use to appraise progress on individual projects? At what level of detail should applied research be directed from above? How are applied researchers best motivated in such laboratories?*

There are only two ways in which the performance of a laboratory can be appraised. One is by detailed investigation; the other by the methods of competition.

Detailed scrutiny of performance is a most important function of the laboratory itself. It is by such intensive evaluation that the laboratory director and the leaders of various projects in the laboratory have to make the decisions that influence the progress of work.

The political decision-makers can and should get their information regarding laboratory performance and capabilities in a regular way through laboratory directors and project leaders. However, they can, and on many occasions they should, call in outside experts to eliminate some of the unavoidable bias and blindness that accompanies self-evaluation.

None of these procedures, important as they are, can replace the checks that one obtains from competition. It has been pointed out in an

earlier section that competition is most important, and that the price of some duplication of effort should be gladly paid in order to have the increased creativity and safeguards against errors that can be provided only by having more than one group working on essential or important undertakings.

In addition to examples adduced above, one may mention the work of the National Aeronautics and Space Administration and the Defense Department in our national space program. It has been argued that the division of our space program into civilian and military branches is uneconomical and undesirable. Indeed, no sharp division of missions can be made, and none should be attempted between civilian and military applications. But the involvement of more than one organization in the enterprise, due to the civilian and military space efforts, is stimulating to all and contributes to the sound progress of our total space effort.

The problem of evaluating the performance of any project would be difficult enough even if it were not impeded by the additional requirement of secrecy, which arises almost invariably in connection with military research and development and with disturbing frequency in nondefense contexts: this requirement is secrecy.

The question of secrecy is of great significance in connection with the development of applied science as a whole. It is both remarkable and significant that the questions addressed to and formulated by the National Academy of Sciences have not explicitly included the question of secrecy. I have included this topic in my response to the question that seemed to me to bear most closely upon secrecy and classification. This is the evaluation of the results of an undertaking in applied research and development. It is certainly true that all relevant secret information can be obtained in every field by appropriate Congressional committees. At the same time, however, secrecy restricts both the scope of the discussion and those likely to participate in it, and a broad discussion is essential to secure conclusions of any degree of reliability.

Fortunately, secrecy does not apply to most of our space effort nor to the greatest portion of our magnificent electronics and associated industries. Removal of secrecy requirements from the major part of nuclear engineering has resulted in an increased capability to build economical power reactors and, at the same time, it has permitted the education of a sufficient number of nuclear engineers. After a decade of essentially open development and education, nuclear reactors have overtaken and surpassed economically the conventional means of energy production. This resulted in the last year in a truly dramatic increase in demand for nuclear power reactors.

It is unclear to what extent secrecy impeded progress in other fields like those of developing nuclear explosives. It is my strong suspicion

that removal of secrecy would permit an important increase in the real security of the United States.

There are actually other respects in which secrecy considerations play an essential role in connection with the development of applied science. One is that secrecy has a distinctly repelling effect upon young people who consider taking an increased interest in applied science. In an open society like ours it is understandable that secrecy should be considered undesirable, at least insofar as one's own scientific activities are concerned. We shall be greatly aided in our efforts to attract more talented people to many areas of applied science, including activities essential to national security, if the requirements of classification can be removed or at least very greatly reduced. A further effect of secrecy is to shield work of low quality from effective criticism. Free exchange of information will therefore contribute not only to speedy development but also to greater economy.

Of course, the question of secrecy has much more broad implications and it does not seem appropriate to delve into all of them when discussing applied science. However, as one who argued strongly for the creation of the first large-scale *secrecy wall* around a Government project in American history, that which (rather unsuccessfully) surrounded the Manhattan Project, I wish to recall the very proper, almost universal repugnance with which it was greeted at the time, to express my growing unease with the continuation, spread and acceptance of the practice, and to warn that it is probably making an over-all negative contribution to our Nation's security; it certainly has an adverse influence on the badly needed growth of applied science in this country.

The last topic connected with secrecy is the industrial equivalent, which is designated "company confidential (or proprietary) information." It has been recognized for centuries that secrecy has harmful consequences with respect to the over-all development of technology. It is for this reason that patents have been established and encouraged. It should be recalled that the aim of our patent policy is to replace secrecy by open declarations and rewards connected with such declarations. It might also be well to remember that several of our Nation's most successful industrial laboratories, such as those mentioned above, almost flaunt their most interesting and potentially most rewarding developments by way of advertising their high quality, and seem to be only strengthened by so doing.

Some of our Government agencies have acted to discourage patents by prohibiting or greatly inhibiting private patent applications resulting from any activity in which public money has been used. It is completely understandable that a Government agency should not wish to support a development, in whole or in part, and they pay royalties on a patent that has been developed during the project. However, application of patents

by members of a scientific team in these instances should not be ruled out altogether. The initiative and ingenuity found in private companies could well be encouraged and rewarded by patents, which could pay off in the sale of patented commodities to users other than the United States Government. The more patents are discouraged, the more secrecy and incomplete disclosure will thrive, to the detriment of an open society such as ours, which relies to a great extent on self-directed groups of scientists and technical experts, seeking rewards in the marketplace.

The formation of a patent policy that sufficiently protects the public interest while avoiding the significant reduction of inventor motivations and incentives is one of the unanswered challenges to makers of public policy in these times of increasing Federal support of pure and applied research. This Federal outlay should be supplemented by tax incentives aimed at stimulating private investment in appropriately broad research in applied science. While our academic community is too narrow-minded to endorse practical pursuits, our industrial establishment is similarly narrow in concentrating on profit without delay or uncertainty; a middle ground should be sought. The Congress would do well to address itself to these very important problems.

6. *What are some of the most significant achievements of applied science worldwide in the last ten years? What was the respective role of basic and applied research in the achievement of these successes? At what point was the possibility of the final goal first perceived, and how was the effort organized toward achieving it?*

Applied science is at least as old as the Industrial Revolution. To provide a background it might be justifiable to go back in the discussion of applied science to the invention of the steam engine, even though the steam engine could not have been invented in the United States, since the United States was just about to come into existence at the time of James Watt. One reason for mentioning the steam engine is the remarkable fact that in this case, and many others like it in those times, applied science preceded pure science. The story represents a remarkable inversion of what one now considers as the usual, even the logical, process of development. First came the steam engine, which is the end product of applied science in this case. In a few decades there followed the understanding of the Carnot cycle, which provides the proper measure for the efficiency of that engine. And it took a few more decades before the principle of conservation of energy was clearly understood so that it became at last possible to define what kind of efficiency was being considered. Pure science came last in this case.

The next big chapter is concerned with electricity. Here the relevant basic discoveries had been made by Michael Faraday and James Clark Maxwell in England. The first significant applications, however, were



made primarily in the United States. It would take too much space to recite here this old and wonderful story. There can be no doubt, however, that the hero was Edison, whose name appropriately appears in the titles of perhaps one half of our large electric power companies.

After a much too brief enumeration of some of the giant steps made in the 19th century we might turn to the first great revolution of the 20th. This is the story of flying. It is remarkable that in this case applied science again took the initiative while pure science lagged behind. Hydrodynamists of the 19th century "proved" that flying was impossible even though it took considerable stubbornness to enunciate such results in view of the performance of the birds.

The Wright brothers were not hydrodynamists. Contrary to popular belief, they were also very different from naive inventors. Having studied and, to a great extent, having understood the behavior of instruments like kites, bird wings, and other airfoils, and having even built and used a wind tunnel, they proceeded toward a solution of the problem of flying along lines that are closely related to the processes that still characterize portions of applied science: empiricism guided by available understanding. Incidentally, the Wright brothers did not use arguments that would attract any big investment. Less than two years before their historic flight Wilbur wrote to the great engineer Octave Chanute: "It would be folly to back such attempts as business propositions. . ." The motivation for the airplane was simply the desire to fly.

A systematic exploration and exploitation of the applied science of flying followed later. Important examples of this activity were the school of Prandtl in Germany and later the school of Theodore von Karman at the California Institute of Technology in Pasadena. The latter's contribution had a decisive influence during World War II and to some extent thereafter, though it seems that recently the purely theoretical aspects of aerodynamics have again come to the forefront at the California Institute of Technology.

The last paragraphs have carried us up to a most active contemporary field of applied science, embracing both airplanes and rocketry. In the development of jet aircraft the United States maintained a strong position up to the last few years. At present, work on supersonic transports appears to be pursued more vigorously in Europe than in America. Important contributions have been made in these fields in Europe. One may mention Whipple in England and Ohain in Germany. In the development of rockets the leading position was occupied a few years ago by Russia. Whether or not the massive effort of the National Aeronautics and Space Administration has changed this dynamic situation is an open question.

There is one big, complex, and exceedingly important field in which the United States held the initiative and continues to make the most

important contributions. This is electronics. Our strength in this field is due to three circumstances. One is the excellence of research in institutions like the Bell Telephone Laboratories and MIT. The second circumstance consists in the great demands and the corresponding financial resources that arise in connection with civilian requirements of communications equipment, and the almost limitless demands that our military establishment is making on electric gear. The third circumstance is the development of electronic computers, which are at the same time consumers for the electronics industry and the producers of new equipment and new lines of inquiry for use by this industry.

The electronic computers are probably the most important result of recent applied science efforts, not only in the United States, but in the world. In making this statement I am putting the development of electronic computers ahead in importance both of development of nuclear energy and of the exploration of space. Briefly the reason for this statement is that electronic computers can perform any intellectual function of the human mind, provided that this function is described in a precise and rigorous manner. Though this last qualification is in fact quite restrictive, it is counterbalanced to a considerable extent by another circumstance: those functions that the electronic computers can take over, they perform much faster, far more flawlessly, and more systematically than can any human.

The consequences of these developments are almost without limit. On the electronic computers we are basing to an increasing extent the process of automation, which replaces the routine work and sometimes the refined work of the brain, just as human and animal muscles have been replaced in an earlier phase of the Industrial Revolution by the greater power and reliability of machinery powered by non-biological energy sources. One must consider automation not merely as a labor-saving technique, but rather as a means of recognizing additional human needs, to create more products and more skilled mechanical labor for human use so that problems can be readily solved today that were not open to an effective attack a few years ago. Thus one can optimize the functioning of production and distribution in industry, as well as provide aid to such areas as detailed military and governmental planning.

In the long run, the application of computers to scientific research of all kinds might turn out to be more important than anything that has yet been mentioned. Here again we are not confronted with the replacement of the research worker. Instead, we can consider computing machines as a challenge for the more intelligent employment of the human brains, which can and should work in effective symbiosis with these mechanical information-processing systems. One of the practical results toward which progress is being made by these means is the calculation, prediction, and, almost certainly, eventual modification of weather phenomena.

The scope and depth of the computer revolution becomes even more obvious if one realizes that a computing machine, by performing functions similar to those that thinking beings have performed before, may serve as a tool of understanding some phases of the thinking process itself. This last point may seem fantastic, and I doubt that the full effect of this development will be felt soon. Nevertheless, I firmly believe that in historic perspective the invention and development of computing systems will be considered as the most important achievement of applied science, and I raise this possibility precisely because of the very close abstract resemblance between the computer and man's extraordinary brain, which has made him the apparently preeminent life form on this planet.

It is highly relevant that in this most important field of cybernetics the United States appears to be maintaining a commanding lead. I should like to recommend that careful attention be paid to the reasons for this lead. This can be best done by a careful questioning of those individuals and representatives of those companies that have brought about the important advances.

In the popular mind, atomic energy is often considered as the outstanding success story of applied science. Somewhat paradoxically, this continues to be the case even though these successes have created many potential dangers, and have persuaded many of our contemporaries to advocate limitation rather than further development of many areas of applied science.

The story is so well known that even a brief recapitulation appears to be unnecessary. It should suffice to mention that atomic energy has established a very potent connection between applied science and power politics, that it has saddled our efforts in these areas with a disturbing heritage of secrecy, that there has resulted an early important and beneficial lead of the United States in this decisive field, and that in the last year the use of atomic energy for the production of electricity passed an important threshold, so that we must consider a massive entry of atomic energy into the field of electric-power generation and associated areas as a firmly established prospect.

Some other comments should be made about atomic energy. One is the important comparison between progress in the United States and progress in the Soviet Union. While such a comparison is vital, it is greatly impeded by Soviet secrecy, which is unquestionably tighter and more successful than our own. All one can say is that the Russian atomic-energy effort in all its phases, military as well as peaceful, is comparable to our own. It is possible that we have retained our early leadership in this general area. It is equally possible that we have lost it.

Another matter that must be discussed, because of the massive public interest that it arouses, is nuclear proliferation, that is, the spreading of applied nuclear science to any and every advanced country. There is

general agreement about the fact that further proliferation will compound the dangers with which we have to live. It is also becoming clear that it is very hard and perhaps impossible to put an effective limit to proliferation. Such means as maintenance of secrecy do not seem to help. One should remember that each of the five present nuclear powers performed its first nuclear explosion shortly after a sufficient amount of fissionable material was available. In no case did lack of knowledge delay the process. Unfortunately, the increasing importance of nuclear production of electric power will make fissionable materials increasingly available throughout the world. Thus there is little hope of stopping proliferation by limiting knowledge, or by limiting the critical materials. Whether a limitation by geopolitical means can be obtained and will be effective is a non-technical question that lies outside the scope of the present discussion. The same holds for the closely related question of whether nuclear proliferation can be stopped by enlisting the enlightened self-interest of those powers that as yet do not possess nuclear explosives.

The prospects for controlled thermonuclear reactions have been discussed in a previous section. This development has resulted in a vigorous and interesting branch of applied science, which is likely to produce some important practical results in the near future, but which probably will attain its declared objective of completely safe, essentially free and limitless nuclear power only a few decades hence. It might be of interest to note that Russian expenditures and accomplishments in this field seem to have surpassed our own in the last few years. It is also worthy of mention that considerable effort is directed toward controlled fusion in quite a few advanced nations throughout the world. It is fortunate that secrecy has been abolished in this field.

To complete the discussion of applied nuclear science, one should mention the peaceful use of nuclear explosives. Such peaceful use is particularly promising in big earth-moving projects, such as the digging of a sea-level canal between the Pacific and the Atlantic Oceans, and as an aid in mining and extractive activities, such as shattering tight oil-bearing formations, thereby increasing the availability of fossil fuel. Investigations during the last ten years have clearly established the feasibility of this effort, which bears the suggestive name, "Plowshare." Plowshare in fact could contribute greatly to the development of new regions of the world. A great complex of outstanding examples can be found on the continent of Australia, on which more than half of the world's best iron ore and aluminum resources are located. At the present time, the full exploitation of these fabulous raw materials is limited by too little population and too little capital. Plowshare techniques could help in this situation, not only in establishing harbors and waterways, but also by providing vital water resources by new types of earth-moving projects in remote regions; it seems to be the appropriate tool with which to at-

tack this challenging problem. It is particularly noteworthy that the use of Plowshare techniques has been retarded not by objective considerations based on technical facts, but rather by certain political considerations that in turn, have complex, non-rational psychological origins. It is in connection with Plowshare that the appropriate political decisions by our responsible policy-makers could produce most important and beneficial results.

Space exploration, which is the latest addition to modern applied science, holds the record in expenditures. In this instance, the initiative has passed at least temporarily to the Russians. The reasons appear to be the following:

At the end of World War II the Russians, recognizing the potentialities of the German V-2 rockets, immediately applied themselves to the perfection of rockets of greater ranges and higher accuracy. Since the Russians felt themselves surrounded by potential enemies, work on rockets of all ranges and payload capabilities appeared to be relevant to national safety. As the technology progressed, the general respect in which applied science came to be held throughout the Soviet bloc made itself felt. Thus the decision to produce nuclear-armed ballistic missiles and, in addition, to mount an extensive space exploration program became easy and logical.

By contrast, the potential enemies of our country appeared to be thousands of miles away, across great oceans. Our monopoly in nuclear explosives gave us a feeling of unjustified confidence. Furthermore, the thoroughly mistaken assertion was made that rockets cannot deliver nuclear explosives from one continent to another with an accuracy such that nuclear fission explosives (rather than the more potent hydrogen fusion explosives) could make themselves felt in a really effective manner. Space exploration was dismissed as visionary and impractical. Thus the whole effort received little emphasis beyond the information we could obtain from men such as Wernher von Braun, concerning the German V-2 and its immediate successors.

The situation changed when the feasibility of thermonuclear explosives was established. A thorough study was initiated in the early 1950's, and the results of this study and the consequent developments saved us from being left behind in the space race. Even so, the Russian Sputniks established an extremely well-publicized and effective leading position for the Soviets.

It seems probable that the United States has by now spent a considerably greater amount of material effort on space exploration than Russia. Even so there is doubt as to who holds the edge in achievements. This is partly due to the early Russian lead. It is furthermore due to the fact that the Russians can more effectively induce the participation of their outstanding scientists in projects closely related to national goals, and it may finally be due to a more effective integration in Russian secrecy. There

can be no doubt that we have published incomparably more scientific information from space exploration, both in volume and in value, than have the Russians. Whether this is due to real superiority or to Russian unwillingness to publish is not clear. It has often been claimed that the Russians are presently ahead of us in the capacity of lifting big masses into space, but that we hold the lead in the sophistication of equipment. Unfortunately it is easier to check on the weight-lifting capacity than on the refinement of the equipment. It is disquieting to see that the Russians have an apparent advantage in the very field in which better information is available.

All other countries have made relatively minor advances in rocketry and space exploration, though efforts have been made both in non-military uses and in military applications. Of the latter the recent Chinese demonstration gives a dramatic illustration.

Many questions have been raised concerning the eventual usefulness of space activities, and a great number of serious misunderstandings have arisen in connection with this subject. Thus one of the most popular topics of discussions is the nuclear bomb in space. This appears to be a thoroughly impractical concept. It is a much more expensive delivery system than those available today. Because nuclear explosives in space have to be decelerated over a period of several minutes in order to descend to earth, the bomb in orbit would not bring about a greater element of surprise than would present ballistic missiles. Effective results of space exploration have to be sought in other applications.

One of these is the well-known and popular field of communications. The fact that amplifiers located in space can transmit high-frequency radio signals along line of sight to earth stations or to other space stations permits the use of very high frequencies in space communication. (These higher frequencies are not deflected in a marked manner in the earth's ionosphere and cannot follow the curvature of the earth as lower-frequency radio waves do.) The importance of short-wave, high-frequency communication lies in the fact that far more information can be transmitted by high-frequency waves in a given time. Incidentally, this is also the reason why short radio waves are needed for television, in which the required rate of information transmittal is quite large.

A second and possibly more important application is connected with surveillance of the earth. We have heard most about the important field of weather surveillance. Indeed, it has become possible for the first time in human history to obtain practically simultaneous information about the state of the atmosphere over the whole globe. This is bound to have a most profound influence on weather prediction quite soon and on other developments connected with meteorology further in the future.

Detailed global surveillance is of course of great importance, not only for civilian purposes, but also in connection with military considerations. It would seem to be very important to pursue this activity on a sound international basis. Something of this kind has been proposed more than a decade ago by President Eisenhower in Geneva when he announced our Nation's Open Skies policy. This policy, which would be important for peace and stability, has not been implemented. There may be little hope of its prompt acceptance by the Communist bloc countries. There is, however, a real possibility of uniting the truly freedom-loving people into a common enterprise directed toward the gathering and dissemination of all information relevant to the insurance of peace. Open skies on a global scale may follow at a later time.

It is not surprising and it is obviously reasonable that all the applications of space connected with near-earth activities have great international interest. This is true in almost equal measure for communications, weather surveillance, and the Open Skies enterprise. At least one conclusion appears obvious. One should indeed bend every effort toward uniting the work of all nations willing to cooperate in these fields. The lion's share of the burden will continue to be carried by the United States. But important economic and, even more important, scientific contributions can come from Western Europe and from the advanced countries of the Pacific. Other countries can and should contribute, partly because of the psychological advantages of "paying their way," and partly because an early agreement on these matters will lead to more significant contributions in the future.

For all these reasons, it is of great and obvious importance to integrate the efforts of the advanced democracies in space exploration. The difficulties lie in organization and in the political field. Here our political leaders could make a contribution that might change history in a major way. In the end, the space effort may become universal and may help to eliminate human strife and disunity.

Many of the fruits of space exploration accrue to pure science. It will be of the greatest possible intellectual significance to gather more complete information about the universe with the help of instruments placed outside our atmosphere which, even before the advent of widespread air pollution, has not been as transparent as one would desire. It is quite understandable, and it continues to be true, that astronomy has played a particularly important role in human thought. At the present time, we may be approaching a situation in which significant detailed information can be obtained about the creation of the universe, an event that, according to present guesses, occurred about ten billion years ago. If such information is to be gathered, it is likely to be obtained in connection with our space effort. At the same time, it is reasonable to supplement space astronomy with ground-based astronomy. Continued

neglect of the latter, relatively inexpensive endeavor will give rise to justified criticism.

Even if one fails to enumerate the many other scientific benefits that can be derived from space travel, one should mention the possibility that some forms of life might be found elsewhere in the solar system, even though recent results seem to have decreased the probability of such a discovery. One should remember that any information concerning another path along which life processes may have developed is of the greatest possible significance with respect to the vitally important enterprise of understanding the biological world, which of course includes ourselves.

One should not conclude a discussion of space exploration without a comment comparing present efforts with future planning. The goal set by President Kennedy to get to the moon before 1970 has been pursued with great diligence and considerable effectiveness. At the same time, there has been little discussion and there exists little effective planning as to the national space program beyond the lunar landing. This is probably due at least in part to the lukewarm support of the general scientific community for almost any type of space program. It is at this sensitive point that our future efforts may be hampered by lack of real unity, by the divergence and dissipation of the politically motivated drive toward manned space exploration, and by lack of broad-based academic interest in this great enterprise. As mentioned above, this constitutes both a result and a symptom of general scientific attitudes toward applied science.

The necessary planning could be coupled with the establishment of a real start in international cooperation. This would be the path of wisdom and statesmanship.

*7. Should the United States strive to maintain a very broad spectrum of capabilities in applied science, or should it concentrate on fewer goals and pursue these with maximum possible effort?*

In general terms, I believe that the pursuit of broadly defined goals is fully justified, and that it is within these broad goals that we have to specialize to particular tasks as the occasions arise. In the answers to previous questions, comments are made concerning the various fields of endeavor in applied science that are now enjoying strong support. In all these fields (with the obvious exception of nuclear-propelled manned aircraft), vigorous continuation of our enterprise appears to be both appropriate and necessary. It is of specific interest to raise the question concerning additional fields that do not as yet enjoy sufficient support.

I would like to mention two of these fields, meteorology and oceanography, especially the exploration of the continental shelves and borderlands. In both, our rather small effort has been expanded in recent times. However, further emphasis is needed. Since the effort that is going to be



necessary will eventually cost billions of dollars, the general arguments for developing these fields have to be clearly delineated.

I have previously noted the connection between our space effort and weather prediction. A further reason why long-range weather prediction is likely to become reliable and effective is the increasing contribution of weather simulation and prediction, which proceeds with the use of electronic computing machines. That the observational information becoming available to us, the theoretical models that are being developed, and the computing procedures that are being perfected will result in improved forecasting within the next few years cannot be doubted; vigorous support of all three efforts can be confidently recommended. What is in doubt is the answer to the question: Will weather predictions become effective over periods of days, weeks, or even over longer periods?

The reason for persisting doubts is that atmospheric phenomena contain many unstable situations. It is in the nature of such instabilities that very small and therefore essentially unpredictable causes can grow into large ultimate effects. These instabilities will apparently set limits to the time span over which weather prediction can remain reliable; but, to the extent that such a limit is reached, another and even more exciting opportunity is opened up.

As explained above, instabilities have the nature of trigger effects in that small causes lead to sizable results. The eventual energy scale of atmospheric phenomena is probably beyond the means of direct human control, even if our most impressive nuclear devices should be employed. To the extent, however, that we can utilize trigger effects, effective control of the weather might indeed be within our reach. One of the controlling features is water-droplet formation, and this mechanism is being employed at present in initial attempts at rain-making. There are other possibilities, however, such as the control of the flow of radiation in the visible and the infrared portions of the spectrum. By these and other means it may become possible to shift the big rain-producing storms in the temperate regions by hundreds of miles and thereby bring extensive precipitation to otherwise arid regions. It may be possible to exercise a measure of "birth control" in the genesis of hurricanes, and it may also be possible to change the length of the growing season by removal or modification of relatively thin snow or ice covers. These examples are not given with any real confidence of their realization in the near future. They are merely stated in order to give an impression of the range of possibilities and the extent of practical consequences in this area.

One point has especially to be kept in mind in the relevant planning: It is unsound to change a system before understanding it in a reasonably complete manner. The present rain-making efforts suffer from the circumstance that one never knows whether a given amount of rain would have fallen without cloud-seeding. Prediction is an absolute necessity if

any process is to be evaluated prior to adoption. This is the more true because weather modification will lead to undesirable, as well as desirable, results. Any effective weather modification will certainly elicit numerous protests. One cannot deal with these protests in a responsible manner unless broad and solid understanding is available.

It is my opinion that before the end of the 20th century weather modification will play a decisive role in both world economics and world politics. It seems most important to give generous support to meteorological research in the United States. Because weather is no respecter of national boundaries, meteorology appears to be one of several areas in which an international effort will be fully justified. We know that in some regions of Europe, in Norway, for instance, meteorology is well developed. There are some indications that the Russians are making a determined effort in this field. It would be highly desirable to put ourselves in a strong position in this strategic area. It would be even better if true worldwide collaboration could be secured in meteorology, with elimination of all barriers of secrecy. We might thereby be able to prevent the Cold War from spreading into the human control of meteorological phenomena. At the same time, it should be realized that the political problems connected with weather control may be even more difficult to resolve than the well-publicized problems connected with nuclear technology.

The second big field in which we will have to accelerate our effort in the next few decades is oceanography, including marine geology. More specifically, we should explore and exploit the relatively shallow regions of the oceans that surround the continents and that include the continental shelves and borderlands. The geological structure of these regions is as yet not completely understood. Mineral deposits of economic significance are to be expected, and among these deposits the petroleum formations have already started to play an important role.

At the same time, the continental shelves are likely to become important in other ways. They present an opportunity of cultivating the ocean bottom and transforming these areas into efficient sources of food. They are of great importance for national defense at a time when submarines can be used for the effective delivery of powerful weapons from short range. Some of our important activities may be best located on the ocean bottom. For instance, the great promise of nuclear-power reactors is accompanied by continued uneasiness concerning very large-scale accidents, in which a nuclear reactor may rupture and disseminate the accumulated fission products. While it is possible to prevent any such accident with great, meticulous, and continued care, it might still be best to locate big reactors far out to sea under hundreds of feet of water, where dilution of fission products by the great mass of overlying water may provide an automatic guarantee against the greatest dangers of malfunctioning nuclear-energy sources.

Some of the discussions in the United Nations have already recognized that national rights in continental shelves extend far beyond the three-mile, or even the twelve-mile, limit. Such rights will not become effective unless one takes actual possession of the ocean bottom and unless the ocean bottom is developed. The continental shelves present the latest and not the least important form of the expanding human frontier. This frontier cannot be exploited except with proper preparation and continued development of the appropriate branches of applied science.

8. *What criteria can be used to decide whether a new technology is "ripe" for exploitation on a large scale? What methods are most effective for appraising the state of the art to determine the feasibility and timeliness of a major technological effort? What kinds of questions should the layman ask of the experts so as best to form his own independent judgment of the ripeness of a new technology, especially if the experts disagree?*

The correct answer to this question would indeed solve all our problems. An answer is just as difficult as the answer to the question of how to differentiate between a right and a wrong political judgment, or between a just and an unjust decision in a court of law. One certainly should not be misled into believing that decisions concerning technical matters can be made in a purely objective manner. Because many arguments have to be weighed and because sufficient data are never available, it becomes obvious that the right people to make the decision are those most skilled in the difficult art of decision-making in the face of uncertainty. It is my opinion that these right people are the responsible political leaders.

At the same time it is also clear that the right decisions cannot be reached without a knowledge of the objective facts. That it is difficult to decide even the objective facts is indicated by the phrase in the above question, "especially if the experts disagree." It should be recognized that experts do disagree. They have a right to disagree, and honesty compels them to disagree when trying to evaluate future potentialities on the basis of uncertain and incomplete data.

There are only three pieces of advice that one can offer in a general sense. One is to give a chance to the capable monomaniac. The Wright brothers were capable monomaniacs; Admiral Hyman Rickover is a living example. Any moderately able and motivated person can succeed in a "ripe" and recognized field, but it takes a special person to cultivate the uncommon seed. It may take real wisdom to recognize the right individual for that job.

A second bit of advice, which has been repeatedly given in this paper, is to employ the aid of a hard-working and intelligent staff. The work of the staff should include the collation of disagreements, together with any questions of detail that might shed light on the source of disagreement between experts.

The last advice is of a slightly more general kind. I suggest disregard of the widespread belief that science and its daughter, technology, can be understood only by experts. It is often claimed, but it is not true, that the world today is more complex than it was in the last century. Progress of science has produced many marvelous things, but one of the most marvelous is the unification and consequent simplification of our views of the physical world. The independent mysteries of physics and many branches of chemistry can now be viewed from a more unified and consequently more simple vantage point than was ever possible previously.

It follows that in investigations by both the Executive staff and Congressional committees one can properly and reasonably insist on a simple exposition of arguments both supporting and opposing a given proposal. Unfortunately the general system of education in the United States does not prepare the average citizen for the task of understanding a scientific discussion. This is most assuredly not due to the complexity of modern science; it is rather due to a deficiency of our educational processes.

One should conclude that sufficient industry in the pursuit of proper decision-making in scientific and technological matters can lead to a level of understanding that will significantly improve the soundness of the decision. Those of our lawmakers who, without being experts in science, have taken the trouble to familiarize themselves with the outlines of scientific problems, and also with the human peculiarities of our Nation's scientists, may make particularly important contributions. Specialization in this field is similar to other important legislative specializations that are not uncommon, such as specialization in foreign affairs.

I want to add a final plea that seems to be fully justified on the basis of the preceding discussions. Education in the physical sciences should be greatly improved in our elementary schools, in our high schools, and even in our universities. Improvements will admittedly be difficult, because teaching has to compete with research and industry for capable scientists. Without such improvement, however, the public understanding and the political judgments concerning applications of science will suffer even more greatly as our civilization becomes more and more technological in its basis. The modern world puts great requirements in these respects upon the leaders of our society. In a democracy, these requirements may seem to be particularly hard to satisfy. But they must be satisfied; furthermore, in satisfying them, I believe that a great amount of intellectual stimulation and satisfaction will be attained.

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In all phases of preparing this paper, Dr. Lowell Wood has contributed in an essential way. The main reason this paper appears under my name alone, rather than with a joint authorship, is that I feel I should take full responsibility for some of its statements on which there is no unanimity in the scientific community.

## A NOTE ON ENGINEERING EDUCATION

by C. RICHARD SODERBERG

### Introduction

This note deals primarily with the issue of generalists versus specialists in engineering. Developments in the educational system for engineering in recent years have concentrated mainly on the education of specialists; the system has not yet succeeded in finding a place for study of the larger issues confronting our technology and society. With the deepening of our commitment to technology as a way of life, there is an increasing urgency for such a widening of the sphere of interest of at least some engineering professionals. The following gives a background to this issue, some of the considerations which now appear to make it particularly important and some suggestions of how the situation may be improved.

The issue is present in all professional education related to fields which have a strong scientific base, but the developments of the recent decades have tended to accentuate it in the engineering fields. Brooks has defined this issue as one of the difficult "dilemmas of engineering education" (1).

Some of the same issues have been discussed by Soderberg in a related context, the position of the engineer among the professionals in American society (2).

Alvin M. Weinberg (3) has discussed the connection between the university and society in terms which are relevant to the present issue. He emphasizes that while society is "mission oriented" the universities are "discipline oriented" with standards of excellence set within the disciplines. The implication is that any attempt at mission orientation in the universities would endanger their essential role. The issue is thus related to the "incongruence" of the structures of the universities and society. Weinberg suggests that the mission-oriented academic and research activities might best be performed in national laboratories or their equivalent.

The development of basic science certainly needs this discipline orientation but its relevance is greatest for those who will go on to creative work within these disciplines. For the larger number of young minds who are destined to play a more direct role in mission-oriented society, an exposure to the larger problem complexes would also seem important. This is particularly true for a field such as engineering. Hence, this purity

of discipline orientation does not seem realistic for professional institutions of learning.

### Some Characteristics of Engineering Education in the United States

The American system of engineering education has been under intensive review over many decades. Its early development started more than a century ago by gradual elaboration of the vocational system of training. The English industrial revolution brought about the emergence of a new type of professional, the engineer-inventor-enterpriser exemplified by Thomas Telford, George Stephenson and many others who created the industrial system and also came to embody the ideals of succeeding generations of engineers. Their outstanding characteristic was utilitarian; technological progress was to be the cure-all for the ills of Victorian *laissez faire* society. There was also a core of anti-intellectualism along with impatience toward scientific sophistication. In the United States we can see this ideal perpetuated in many of the prominent industrial leaders at the beginning of this century. Henry Ford and Thomas Edison are examples.

The educational system which developed to meet those immediate utilitarian demands served the emerging industrial society quite well for many decades. Its weaknesses began to be apparent in the United States by the end of World War I. The long process of reform of engineering education actually began in the 1920's. Much of this development was originated in industry particularly on the part of mature corporations such as General Electric, Westinghouse, Bell Telephone and others, which simultaneously supported research on an increasing, if still modest, scale. Even at the end of the 1930's, graduate work in engineering, particularly to the doctorate, was still on an insignificant scale.

It was not until World War II that the true role of graduate education and research began to be more widely appreciated. The statistics on doctoral degrees in engineering show that in the early 1940's, with the exception of chemical engineering, there was only a handful of engineers with the doctorate degree in the Nation as a whole. Even in the late 1940's the number of men with doctoral degrees, now approaching 10,000, was a matter of a few hundreds. While the over-all educational system in engineering had expanded and matured, it was still based on the four-year undergraduate degree, with much of the professional education centered in the transmission of practice (often outdated) from one generation to the next.

The reasons for the sudden change of values during World War II are readily understood. The war brought in its wake technological development tasks which required a new level of scientific knowledge as well as inventive ingenuity and industrial know-how. The Manhattan project,

the development of radar, communications technology, advanced development of aircraft, and many other undertakings of lesser scope required a degree of scientific sophistication which so far had not been part of the engineering tradition. It also brought into the process of technology talents in physics, mathematics and all the physical sciences, which until then had been applied only sporadically to engineering.

The impact of this experience upon the engineering profession has been profound. Among some of the men of the older tradition, there developed a feeling that "science" had somewhat unfairly stolen a march on "engineering" and captured most of the glamor. The semantic confusion of the two terms also brought about a certain irrational resistance to the very reforms which the situation indicated to be necessary.

The more important and much more constructive response led to a general strengthening of the educational process by graduate studies and by research. The growth of graduate education in engineering during the last two decades is truly phenomenal. In the decade from 1955 to 1965 engineering Ph. D.'s rose from 7 percent of all Ph. D.'s to nearly 13.5 percent of all Ph. D.'s. By 1975 engineering Ph. D.'s are projected to become 20 percent of the total and will exceed those in both the physical and in the biological sciences.

The leading schools of engineering now have programs in which teaching is combined with purposeful activity in research and through applications of science to technology. There is emerging a new university concept with far deeper involvement in a Nation's problems than in the earlier framework. But this process has only started; and in engineering, in particular, new developments will be needed. For example, there is a lack of organic cohesion between the undergraduate engineering curricula, still bound by the traditions of the older system, and the recent and more vigorous graduate activities in study and research. There is also an enormously wide diversity along the spectrum of the 150 odd schools of engineering of which not much more than a dozen, at most, can be said to have the background and resources for the new mode of activity. This diversity is not all bad; it is also a necessary part of our educational system, since continued experimentation is one of the best means for retaining academic vigor and since the spectrum of activities and the educational needs of the engineering profession are so varied. The system is characterized by dynamic rather than static equilibrium as the institutions advance in size and complexity. Furthermore, the range of variety of talents, intellectual styles, and motivation required for engineering is far greater than for the basic sciences.

It was almost inevitable that this development should become oriented mostly towards the training of specialists in the applied sciences related to engineering, since only a small part of the total activity in technology can be pursued effectively in the traditional academic environment.

These specialists also fill an absolutely essential need in the operations of technology. The question is how the equally important need for generalists can be satisfied, not only for the process of technology itself but also for the larger social needs, including the administration of institutions whose functioning depends more and more on technology.

To anyone who has been exposed to the larger problems of the technological process, it becomes apparent that in spite of the many gains in engineering education we still have traversed only a part, probably the easiest part, of the road. Our engineering specialists and many of our teachers live out their lives in an uncluttered world of deceptive simplicity, isolated from the general messiness of real life. Anything that cannot be brought within their relatively simplified conceptual framework tends to be relegated to a limbo without interest. The system is also a self-generative one, because teacher education is necessarily part of the process. The educational system develops its own standards for promotion and ideals of excellence and merit.

It is well to bear in mind that, the deficiencies in engineering education notwithstanding, the American industrial system shows no impairment of its competitive position in world markets. We manage to maintain, in most fields, a level of productivity and of innovation which is the envy of the rest of the Western world, enabling us to compete in many fields in spite of wage and salary rates much higher than the rest of the world. There are glaring exceptions, related mostly to fields where the American genius for mass production and organization does not get full scope. Marine transport and shipbuilding are examples, and there will be others in the future.

Much of the deficiency in engineering education may be made up in subsequent on-the-job training and experience, especially in the large and diversified organizations which are so characteristic of applied technology in America. In this connection it is noticeable that the most backward industries are those which are highly fragmented, and which are thus unable to make effective use of the products of the American educational system, because they cannot afford the added costs of on-the-job education. This also tends to be true of the public service, especially at the State and local level, where the technologies of public services tend to be very backward.

Certainly, another aspect of these lags, however, has to do with social and economic motivation, which we shall now discuss.

### **The Motivations of Technological Society**

Modern society in the Western world, particularly the United States, is characterized by its commitment to technology as a way of life. By the intimate interaction of technology and science, man has reached the stage where he can satisfy material needs to an extent which was never thought



possible before. Within the developed societies, he can provide for a high standard of living for all but a minority of the population; it remains to be seen how rapidly this newly won power can be transferred to the poor developing societies. Not the least significant aspect of the new power is the capacity for nearly unlimited destruction in time of war.

Up to now Western man has been able to gain this power without full realization that the development as well as the exercise of the power required fateful social choices. The making of these choices represents the true challenge of our age.

Free societies such as the United States will tend to avoid difficult social choices, leaving the free enterprise system a maximum scope. The supreme exception to this attitude occurs in time of war, when survival may be at stake. As a result we have succeeded in making the technological system fully effective only in making war and in operating industry for the private market. This is no small achievement, but there are many reasons for believing that we now face a whole array of important tasks where the private market as presently constituted is an inadequate allocator of productive and technological resources.

It is also worth reiterating that our technological and business system as such is indifferent to the many moral and ethical issues involving difficult collective or social choices which are more than the least common denominator of private value systems. Neither technology nor science will by themselves provide anything but means toward ends. The ends themselves must come from other motivations. To an increasing degree private interests and benefits come into conflict with collective values which cannot be measured in the marketplace. The technological system as such lends itself impartially to the destruction or the improvement of our environment, to the production of trivia and frivolities or of utility and beauty.

The effectiveness of the technological system for war making is, of course, one of the sources of national strength and power; but it also represents a fateful combination of circumstances. The periods of war have been periods of technological zest and vigor, contrasted with the listlessness of the period between the two world wars. The period since World War II has a different cast; it has been characterized by a more or less continuing cold war, interspersed by conflicts such as Korea and Viet Nam. There is a frightening suspicion that the major developments of technology and even science have been stimulated mainly by the requirements of war making.

One of the most challenging tasks, therefore, is to bring into periods of peace the kind of zestful motivation and clear-headed thinking which we have to some degree achieved in the pursuit of military capability. There are many reasons for assuming that these problems of peace are now beginning to take on an increasingly important role. One example is the decline of industrial vigor in many of the older technological fields, which

are still essential to our well-being. Railway passenger transportation, marine transport and shipbuilding and mass passenger transport into and out of our cities are examples. The rapid technological advances in the newer fields have taken away the premises of profitability in the older ones. Certain industrial complexes, even though essential for the functioning of society, are left behind as intellectual slums, stagnating in contrast with the newer fields. This phenomenon, resembling the decline of our city centers, is particularly pronounced in the United States due to the vigor of our system of free enterprise as well as to our relatively higher preoccupation with advanced technology.

The present period is also characterized by an increasing concern for the irreversible deterioration of our environment, the contamination of air and water, which have occurred as byproducts of rising population combined with even more rapidly rising standards of private consumption. No less important is the realization that we have up to now often permitted technology to destroy our sensitivity to ideals of beauty and civility.

Technology and science are essential to the resolution of these problems; most of us believe that we have the ability and physical means to effect a solution, once the difficult stage of social choice has been resolved. But this choice may be a bitter one from our habitual point of view: it may involve a radically different balance between certain personal freedoms and public values, from the size of families to the mode or rate of application of new technologies. It will inevitably enhance the role of all levels of government, especially the Federal.

The process of technology itself presents us with problems of its own. The never-ending drive towards larger and more effective systems and units is now beginning to confront our industrial system with investment choices which come in such large individual packages that private enterprise finds it difficult to assemble the resources required or to assume the risk of so many eggs in one basket. In burgeoning industries like computers and nuclear power plants even giant corporations are running into problems of cash flow in providing the investment necessary to meet existing demand for the products of a new technology. An even more striking example is the pending development of supersonic air transport, the SST program. The cost of development here is so large that it is assumed that the commercial airlines and the airplane and engine manufacturers could not supply more than a fraction of the required capital. The predictions of ultimate profitability are naturally uncertain, not the least because we do not yet know the limitations imposed by noise and sonic boom and the social and psychological reactions to them. Hence, the principal justification for initiating the program must be related to the doctrine of inevitable progress and to the matter of national prestige and the international balance of payments rather than direct economic motivation. It remains to be proven that our society is organized to under-

take such tasks effectively or to evaluate progress in the absence of the military justification.

The space venture is another major task, which does not even have tangible commercial end objectives. It offers fascinating vistas of new knowledge about the universe and can thus lay claim to sincere support from the scientific community. But this was not the primary motivation. Without passing judgment on this program, we believe it must be classified with military ventures. Its initiation was certainly an outgrowth of the cold war. If the strategic confrontation between the superpowers hopefully recedes into the background, there will have to follow a reappraisal of relative urgency of the various space objectives, in competition with the multitude of other public undertakings relating to more immediate social goals.

The foregoing discussion of technological motivations relates to engineering education because the recent upsurge in graduate engineering education as well as the reorientation of undergraduate education, are partly the result of our preoccupation with national security goals. Well over 75 percent of the total national investment in technology—public and private—is directly related to national security goals and 65 percent of the research support provided to engineering schools comes from the Department of Defense and from the National Aeronautics and Space Administration. The attitudes and values associated with these primarily physical-science limited technologies have penetrated deeply into the attitudes and values of faculty members and students in our most prestigious engineering schools. Electronic and aerospace engineering attract the best and the largest number of students. The products of these schools tend to be insensitive to considerations of the social, political, and economic factors which condition the acceptance and diffusion of technology. These attitudes have both virtues and limitations. On the one hand, today's engineers have much bolder and less inhibited views of what is technologically possible. They are more confident in dealing with complexity, in utilizing science, and in overcoming technical obstacles. But on the other hand, they are impatient with economic constraints and customer-oriented requirements, and they are almost too confident that technology will find a way in such areas as education. Even within technology they tend to be discipline-oriented rather than problem-oriented, to think in terms of technologies looking for applications rather than social or economic problems in search of technical solutions.

### **Some Means for Strengthening the Professional Elements of Engineering Education**

Granting that the technological as well as the social system would be aided by engineering professionals of broader background, what can be done to aid in their training and inspiration? While the academic process

must be the real base for this education, the demand for breadth rather than depth presents a basic contradiction. This dilemma has not yet been resolved. It seems superfluous to assert that the main part of a man's education must be education in depth; he has little claim to education unless he has participated in the experience of penetrating deeply into one or at most a few fields. In a time when theoretical knowledge is increasingly the foundation of technology, the engineer must be able to talk the language of the scientist, and to assimilate new developments in science throughout his professional career. He must be able to keep in touch with the research frontiers, and to "impedance-match" between these frontiers and technology.

The search for a meaningful synthesis of the process of technology suitable as a basis for professional education, is no new task. Every age has formulated its own conception of this synthesis. Before the present epoch, it was taken for granted that the young engineer, to earn his keep, had to possess a certain competence in design, manufacturing, sales, and operation of certain topical categories of machinery, such as textile manufacturing, power generation, etc. In the early framework of half a century or more ago, this demand had a degree of justification; the pace of new developments of the old fields, and the introduction of new fields, was slow enough so that there was some meaning to the idea of transmitting professional know-how from one generation to the next. World War II and the acceptance of applied science as a full-fledged partner to engineering, changed this situation completely. The pace of change is now so rapid that each generation of engineers must expect to deal with wholly new problems. The real test of professional education is the ability to deal with problems and issues which were unknown when the neophyte was at school. The natural response to this is an increasing emphasis upon basic knowledge and the conceptual structures which organize it. The result is that engineering education has become less mission-oriented and more discipline-oriented; some schools of technology, such as M.I.T. for example, are developing in the direction of universities. This transition is itself symbolic of the very issues under discussion.

It is possible to take two views of advanced professional education. In one the student is regarded primarily as an apprentice who learns by doing, and learns the tools of his profession from other experienced professionals in his field. In the other view the educational process is one in which each student is exposed to many different specialists in the various disciplines relevant to his profession, so that ultimately the general knowledge and skill of the present generation is formed from the combination of the specialized knowledge of the previous generation. Thus it is the task of each specialist to sift his own discipline for those aspects having the greatest generality and applicability and to pass these on

as part of the general intellectual equipment of the professional student. In this view, each generation of students stands on the shoulders of many specialists of the preceding generation. Thus the student should end up knowing more about neighboring or relevant disciplines than his professor in the same discipline. The budding physicist would know more mathematics than his physics professor, and the engineer more physics than his engineering professor. This is possible because as knowledge in the various disciplines advances it can be better structured and presented. New knowledge gained laboriously can be presented succinctly, thus enabling the student to absorb from many specialists what it took each of his teachers a lifetime to develop and assimilate. At least this is the theory on which professional education in engineering, medicine, business, and education increasingly depends. Ideally under this scheme the student comes to his profession with far more sharply honed intellectual tools than his predecessors—the tools forged for him by research at the frontiers in numerous relevant disciplines.

In conformity with this scheme of professional education, the faculties of the professional schools are increasingly populated with people trained in and doing research in related academic disciplines rather than being themselves members of the profession for which they are teaching. These may be either the traditional disciplines associated with the profession—e.g. structural mechanics in engineering, or pathology in medicine—or they may be wholly new disciplines at the frontiers of recent pure science—e.g. information theory or solid state physics in engineering, or molecular biology or biomathematics in the case of medicine, or economics or sociology in the case of business.

There is a difference, however, between the specialist on a professional faculty, and the specialist in the corresponding academic department in a graduate school. The disciplinary specialist must be sufficiently in tune with the professional environment in which he teaches, so that he himself is capable of forming a bridge between his discipline and the profession, just as the professional forms a bridge between relevant sciences and society. Thus the physicist or mathematician in an engineering school must be an individual who can talk as an equal to his colleagues in pure mathematics and physics, but who can also talk with real respect to his engineering colleagues and have some interests and goals in common with them. It is this "bridging" capacity which is the hallmark of the successful "applied scientist."

Where does this leave the bona-fide professional in the faculty of a professional school? Where does he practice his profession, and how does he relate this to his teaching function? In the case of medicine the teaching hospital forms a natural meeting place for the clinician and the scientist, though not always successfully. In engineering, however, we have no real counterpart to the teaching hospital.

Industry and business are usually considered the potential counterparts in engineering to the teaching hospital in medicine. As observed earlier, industry has indeed participated actively in the development of engineering education. Many industries have created first-class educational programs for their young employees, and there have been many successful cooperative ventures of education between engineering schools and industries. However, these ventures have had, mostly, the character of vocational and local indoctrination. It has been more difficult to fit them into the modern demand for creative activities of a more sophisticated kind.

The nearest equivalent is perhaps the management consulting firm run by business school faculty members, or the small "spin-off" company run by engineering faculty members, but the involvement of students in such enterprises is more accidental than purposeful.

Throughout their history the engineering schools have experimented with a variety of devices, to provide a more meaningful professional environment in engineering education. It might be worth listing and commenting upon some of these (4).

**1. *The adjunct professor who spends the majority of his time in industry or consulting, but teaches one or more practice-oriented courses***

It is hard to keep such individuals sufficiently intimately involved with the rest of the educational enterprise. Only in particular regions of the country where a "Route 128" type industry has grown up is ready mobility between the academic and industrial environment really possible. Even in this case the adjunct professors are often not "real" engineers engaged in design or systems management, but are more likely to be engineering specialists working around the periphery of design groups, but really engaged in the same sort of consulting as the full-time professors. Furthermore, the adjunct professorship alone does not guarantee a meaningful engineering experience to the student.

**2. *The use of the engineering faculty or separate institutes within the engineering faculty to develop or provide technical services for the university itself***

With the increasing importance of physical technology in both education and medical care, there is a great new opportunity for engineering involvement which directly serves the mission of the university rather than some outside client. Thus an environment more closely analogous to that which automatically exists in large industry can be provided. There may be similar opportunities in the design of large instrumentation for pure research, as in the design and construction of accelerators or radio-telescopes. With the current large building programs in universities, there

would appear to be an opportunity for engineering services to the university in establishing technical specifications and even in designing the technical sub-systems. The difficulty with this is that it cannot be done easily with full-time faculties, both because of inevitable schedule conflicts, and because the tradition of faculty independence or academic freedom militates against faculty members serving in a supporting capacity to other faculty members or to the university administration. This has been explored in a few of the large universities in connection with computers and high energy accelerators.

**3. *The creation of cooperative research or development projects involving partnership between universities and outside mission-oriented institutions such as industry***

Several of the early computers were designed and built under such arrangements. Even quite recently the SDS 940 time-sharing computer developed out of cooperative arrangements between scientific data systems and the Berkeley Electrical Engineering Department, and the University of Illinois is cooperating with the Burroughs Corporation in the design of a new "super-computer" of somewhat novel concept. The Advanced Research Projects Agency of the Department of Defense has supported three "coupling" programs involving partnerships between an industry or Government laboratory and a university in some phase of materials technology. It is still too early to assess the success of these programs. The difficulty with all such collaborations is their cost. Judged purely as education they do not make economic sense. There must be valid market for the end product to justify the high cost, and it is doubtful whether such projects could be sufficiently proliferated to provide a general solution to the problem of improving the environment of engineering education.

**4. *The creation of multi-disciplinary Federal mission-oriented research institutions in close association with university campuses***

The Lincoln Laboratory of M.I.T., the M.I.T. Instrumentation Laboratory, the Livermore Laboratory of the University of California, and the Jet Propulsion Laboratory of the California Institute of Technology are all examples of such institutions, although their relationship to the campus is often limited. Dr. Teller has advocated a much more explicit educational component of such laboratories, with graduate student and faculty involvement in its operations, and he has actually initiated an experimental program between Livermore and the campus of the University of California. A similar association between the Oak Ridge National Laboratory and the University of Tennessee is also being tried. Cost is, of course, also a problem with such experiments, but much more could be done to increase the educational opportunities in existing Federal laboratories.

### **5. *The subcontracting of suitably selected design problems by industry or public bodies to engineering departments***

Some interesting experiments along this line have been conducted at the Thayer School of Engineering at Dartmouth. It takes a good deal of missionary work to effect this kind of association, but the Dartmouth experiment shows that it can be done under suitable circumstances.

The advent of computer technology may present a new opportunity for more realistic involvement of students in engineering problems. Computers permit the relatively inexpensive simulation of large-scale engineering systems, and the trial of many alternate designs. At the same time, by permitting much more elaborate calculations, they make it possible for students to make designs and analysis on more realistic problems. Nevertheless, computer simulation cannot involve all aspects of the technology transfer process, and to exclusive experience with this particular aspect of design may tend to minimize the more inventive and intuitive aspects of engineering.

It is doubtful whether there is any single solution to the discipline-problem dichotomy in professional education. There is certainly need for more experimentation, and better documentation and evaluation of the experiments that are tried. It is also possible that the study of the innovation process and of technology transfer as scholarly problems in their own right will eventually lead to new professional courses which will themselves make an important contribution to the professional orientation of the professional student.

The recent years have been characterized by a growing public concern for some of the social and economic problems, which have been brought about by the growth in industry and population. It is my conviction that, given adequate attention within the university and adequate financial support, these problems can serve as effective vehicles for broadening the educational process.

The transportation of people within and between the developing super-city complexes is one such example; the deterioration of our environment of purity of air and water is another. The preparation of such issues for political and economic action is an important task for the universities and particularly the mature engineering schools which can serve as challenging vehicles of professional education. The experience at M.I.T. and other institutions in connection with studies such as the Northeast corridor transportation project has given encouragement to this kind of educational venture. Such exploratory studies involving all phases of the problems appear to have great promise as a source of inspiration to many students and teachers. This applies not only to graduate students but to undergraduates, even freshmen as well. Some of the earlier work at M.I.T., related to the search for educational experiences in imaginative design, has also given encouraging results.



In a series of papers William W. Seifert has presented the setting of the activities at M.I.T. on the transportation problem. The most relevant of these papers in the present context was presented a year ago (5). The following points are extracted from this paper.

In planning ways to introduce the problems of transportation to engineering students, it soon becomes apparent that the problems go beyond any engineering discipline, in fact they go beyond engineering itself. The technological problems of transportation are inextricably entwined with social, political, and economic problems. Transportation thus represents interdisciplinary activity in its most complex form. Up to now, in spite of frequent professions, American universities have had relatively little experience or success in providing training for areas that spread across the lines of several disciplines; particularly if some of these disciplines are in engineering and some in the areas of sociology and political science. Dr. Seifert presents the following summary of the experience of the recent years at M.I.T., which gives an insight into the spirit of the undertaking.

For the past five years, we have been gaining experience at M.I.T. with a broadly interdisciplinary subject which could serve as a very effective means for focusing and integrating the experience of a number of students interested in transportation. This subject is designed to give students a realistic experience in the design of complex systems. In this subject, the students direct their attention during the entire term to the preliminary design of a system to perform some particular broadly-stated mission. This year, and last year, the problem has been selected from the transportation area. Last Spring, the group designed a specific system to provide high-speed ground transportation in the Northeast Corridor of the United States between Boston and Washington. The group included faculty and students from Civil, Mechanical and Electrical Engineering and from City Planning, Political Science and Architecture. During the term, the students heard lectures which provided them with information on the status of many of the more advanced concepts of ground transportation. To carry out their own design, they organized as an operating team, with groups given responsibility for vehicle design, system layout, terminal design, and political implementation, to name a few. Each group selected its own group leader, and the class as a whole designated a student to serve as over-all project manager. The goal of this project was to develop the preliminary design for a complete system. At the end of the term, the students presented their design to an audience composed of interested members of the faculty and of over one hundred invited guests representing various governmental and industrial groups interested in the problem. As a final requirement, the class prepared, without faculty editing, a report outlining their design in considerable detail. Each class, thus far, has produced a design which has held up well when compared with similar studies made by groups with considerably more experience.

This year, the class has been struggling with the design of a public transportation system capable of providing relief on a relatively immediate basis for the transport problems of a metropolitan complex such as Greater Boston, while offering the evolutionary potential required for it to meet effectively the needs of the area in the last decades of the century. This is a difficult problem, indeed, and the students obviously will not produce a complete solution. None the less, I am sure that the experience they are getting in working as a member of a team will be of very great benefit for those who decide to accept positions in the field of transportation or, in fact, in any field in which the problems require personnel with a broad systems training.

We still have much to learn about the most effective form for such ventures in education. It is a very expensive type of education, requiring

substantial financial resources and much greater faculty time per student taught. Thus far this type of experience has been made available to no more than a few of the best motivated students. A whole series of criteria have to be fulfilled: the problems must be topical, relating to some recognized and urgent social need; the problems must be placed in their full dimensions in terms of disciplines, so that the behavioral and social sciences and the humanities must be represented along with economics and engineering. Most important of all is to have at least some of the leaders of the teaching staff directly involved in the national planning and execution of the projects. The educational return is the greatest when the problems are in their early exploratory stage. Later and more ambitious programs of execution are less likely to be successful within the university environment. It is in this latter area that institutions of the type represented by the national laboratories will find their role.

This type of experience cannot be regarded as a substitute for study in depth of the individual disciplines. Experience shows that such programs are more meaningful for a certain type of student, often the highly imaginative and intellectually well endowed. Furthermore, it cannot occupy more than a small fraction of the total curriculum, for the student must acquire the intellectual tools of his trade, and it is probably too inefficient for him to learn them all in the process of problem solving. A few people have made the radical suggestion that all engineering ought to be taught by the case method with the acquisition of tools being relegated to various forms of self-instruction such as teaching machines, computer-aided instruction, or guided reading, to be conducted as required for the particular problem. Although such a scheme would have the virtue of motivating the theoretical learning, it would be so poorly matched to the intellectual structure of the knowledge that the learning process would lose coherence.

### Conclusion

The history and background of technology and of engineering education have tended to place increasing emphasis on discipline-oriented studies and technical and scientific specialties. A long period of national preoccupation with advanced developments for military purposes has tended to isolate some of our most able professionals from the urgent social problems of today. Even though the skills in technology and science are essential to the solution of these social problems, many of the best minds in the engineering profession tend to be indifferent to them.

Up to now the national emphasis has concentrated mainly on those activities which tend to perpetuate this situation, with much less support for the broader social problems. As a result of this, only a few of the well-established universities have been able to carry on such activities. More-

over, within all universities the younger staff members seeking promotion are conscious of the risk of engaging in interdisciplinary activities.

As national support for the urgent social problems created by technology becomes a necessity, Government agencies will be confronted with the execution of these tasks. In anticipation of this, it seems equally urgent to encourage the type of educational venture exemplified by the activities in transportation which have been discussed above.

The National Science Foundation, as an example, could foster efforts in a number of the broader socio-technical areas by awarding research grants and fellowships and by the sponsoring of summer workshops on appropriate topics. Eventually it might be possible, perhaps through some of the foundations, of establishing chairs in some of the areas where technology and society interact strongly. This would help to give stature to interdisciplinary activities and would allow a few outstanding teachers a free hand to carry on and to develop their ideas as to how engineering education could best be developed for those students who are concerned with applying technology to meet the needs of society.

The present epoch is confronted with an abundance of such multi-dimensional issues. To give some of our best minds the opportunity to participate in team efforts to better understand these problems—to put the problems into the service of education in the early exploratory stage—would thus seem to be one of the urgent tasks of professional education related to technology.

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## SOCIAL PROBLEMS AND NATIONAL SOCIO-TECHNICAL INSTITUTES <sup>1</sup>

by ALVIN M. WEINBERG

### Introduction

During the war, and for more than 20 years thereafter, our gigantic Federal scientific apparatus was and has been focused upon technological problems. Nuclear weapons, nuclear reactors, and space rockets are some of the technological miracles that have flowed from an unprecedented application of new science by our Federal Government. The instrumentality for achieving these miracles typically has been the large, mission-oriented laboratory. Some of these laboratories, like Goddard or Lewis of the National Aeronautics and Space Administration, are Government operated; some, like Los Alamos or Oak Ridge, are operated under Government contract; some, as in the development of the intercontinental ballistic missile, have been privately operated for profit, but have been supported by the Government. In every case the laboratories have brought together powerful, interdisciplinary teams, with representatives from all fields of science and engineering, to attack big technical problems that the Government regarded as essential.

The products of these laboratories in the past have typically been devices: hydrogen bombs, or power reactors, or space vehicles. And, indeed, there are many devices or processes that obviously remain to be invented, and to which our society will undoubtedly devote much of its energies. Some of these devices are the economical breeder reactor, the artificial kidney, the electric nonpolluting automobile, and the underground electrical transmission line. Yet a basic change is discernible in the character of the problems that our Government's scientific apparatus is now being called upon to solve. Rather than being concerned so exclusively with technology and devices, we are now calling upon our experts to try to eliminate the great *social* problems that disfigure our existence and impair the quality of human life. We are trying to mobilize around infinitely complex social questions like race relations, or the rise of crime, or the decay of our cities, or the improvement of our mass education. We

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<sup>1</sup> The first part of this essay is an extension and elaboration of views I expressed in "Can Technology Replace Social Engineering?", presented at The University of Chicago, June 11, 1966, and published in *The University of Chicago Magazine*, LIX, 6-10 (October 1966). The paper was subsequently published by the *Bulletin of the Atomic Scientists* XXII, 4-8 (December 1966).

now concern ourselves with the deterioration of our environment caused both by the garbage of our civilization and by the growth of our population; with our resources of water and of air; and with the auto junkyards that mar our countryside. All these domestic questions are set against an ominous backlog of unresolved issues with strong international implications; counterinsurgency and arms control and civil defense; or foreign aid; or the overwhelming problem of population control in the world's underdeveloped and overpopulated countries. These great and pressing questions, upon whose resolution the future stability of our society depends, are largely social. In trying to cope with them, we tend naturally to approach them as social problems. We mobilize the political and socio-scientific resources of our Government to devise better laws or more attractive economic inducements to coerce or cajole or otherwise induce people to behave more responsibly. If Negroes are barred from certain schools we desegregate our schools by legislation. If cars pollute the Los Angeles atmosphere, we pass laws requiring each car to carry a catalytic afterburner.

We are handicapped in dealing with social problems not only because they are inherently so difficult but also because our resources for attacking social problems are incomparably smaller than our resources for attacking technological problems. Thus, there is a severe mismatch between the Government's magnificent scientific resources for attacking *technological* problems and the seemingly *social* character of the problems that the Government is trying to solve. It is almost as though the Government, in addressing itself sharply to these broad social questions, finds itself the victim of a kind of technological obsolescence. Its laboratories, and its contractors, dominated so much by their original military missions are by-and-large hardware-oriented, whereas many of the problems that must be solved are social.

Is there any way of bringing the largely technological resources of the Government to bear on these social problems? I believe there are two related avenues to achieving this end. In the first place, as I discuss in Part I of this essay, many problems that are traditionally viewed as being primarily social possess stronger technological components than one at first suspects.<sup>1</sup> They therefore may admit to technological palliatives, or even "fixes," which hopefully can buy the time necessary to get at the cause of the social problem. And secondly, as I discuss in Part II, to the extent that these social problems admit of technological solutions, I believe the country's technologically oriented instrumentalities—its laboratories and hardware contractors—can be modified and then mobilized to find partial solutions to deeply important social problems.

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<sup>1</sup> This point has been stated very well by Richard L. Meier, *Science and Economic Development: New Patterns of Living*, 2nd Ed., 144, The M.I.T. Press, Cambridge Massachusetts, and London, England (1966).

## I. The Technological Fix

To illustrate my approach, I shall describe two ancient social problems—widespread poverty and all-out war—which technology has resolved to a remarkable degree. To take widespread poverty first, the traditional Marxist view (which still dominates much of the world's thinking on the subject) holds that poverty can be eliminated only after far-reaching, profound social changes have taken place. But in expounding this view, Marx seems to have underestimated the role of technology. For the brilliant advances in the technology of energy, of mass production, and of automation have created the affluent *capitalist* society. Technology has provided a "fix"—greatly expanded production of goods—which enables our capitalist society (with modifications to be sure) to achieve many of the aims of the Marxian social engineer without going through the social revolution Marx viewed as inevitable. Technology has converted the seemingly intractable social problems of widespread poverty into a relatively tractable one: it has provided the surpluses that enable most of Western society, rather than a tiny part of it, to enjoy affluence. This is not to say that social changes have not been necessary in achieving this state of affairs; it is merely that the degree of social change has turned out to be far less than the social revolutionaries anticipated, and for this we have technology to thank.

My second example is war, at least all-out war. Here the evidence of the past 15 years convinces me that the hydrogen bomb is the nearest thing to a technological fix for large-scale war.<sup>1</sup> The hydrogen bomb has greatly increased the provocation that seems to be needed to precipitate large-scale war, not because men's motivations have been changed, not because men have become more tolerant and understanding—in short, not because changes in social attitudes have been achieved—but rather because the appeal to the primitive instinct of self-preservation has been intensified far beyond anything one could have imagined prior to the H-bomb. One simply cannot deny that the Soviet leaders, having recognized the overwhelming force of the H-bomb, have adopted a much less militant attitude than they maintained before the Cuban crisis. One can only hope that the Chinese leaders, as they acquire familiarity with H-bombs, and as their society tastes the fruits of affluence, will also become less militant. The main point is that the hydrogen bomb has made all-out nuclear war an irrational instrument of national policy. Insofar as one can assume that the world is in the hands of rational leaders, the hydrogen

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<sup>1</sup> I realize that my view of the role of the H-bomb as peacemaker is not held universally. Yet, there are surprisingly many political scientists who now adhere to this rather uncomplicated point of view. See, e.g., O. R. Young, "On the Nuclearization of International Politics," p. 101 in *On Minimizing the Use of Nuclear Weapons*, Research Monograph #23, Center of International Studies, Princeton University (1966).

bomb has imposed an unprecedented kind of peace on the world. To be sure, the peace is fragile. To this one can only repeat that we have avoided all-out war thus far. The H-bomb appears to be buying us time that we desperately need to control population (incidentally, with help from a technological fix, the intra-uterine device) and, with advanced technology, to raise living standards in the underdeveloped countries. Such an increase in living standards, by making peace much more desirable than war, ought to help stabilize the metastable, bomb-imposed peace.

### ***Technological Components of Social Problems***

Can we identify technological components in some of the social problems to whose resolution our Great Society is now dedicated? If these technological components are sufficiently well defined—if, for example, they find their expression in the invention of a single device—then to this extent the underlying social problems become more tractable than is the case if we cannot identify such technological components. For, in general, a technological invention is easier to make and put into use than is a social invention. Many fewer people were involved in our decision to go ahead with the Manhattan Project than were involved in our decision to adopt the pay-as-you-go income tax. In consequence, it was easier to start on the atomic bomb than to modify the income tax laws. The beauty of a technological resolution of a social problem is that technology usually doesn't have deep semantic overtones and connotations. A technological solution to the water problem that supplies more water is *a priori* easier to put into effect, and evokes fewer hostilities, than does a solution that requires individuals to reduce the number of baths they take. On the other hand, a technological solution to a social problem may create other problems: pesticides enable us to grow more food but they leave dangerous residues; nuclear fission gives us cheap energy but puts coal miners out of work. But in this respect a technological solution is little different from any way of dealing with a social problem: effecting social change, by any method, almost always presents us with new and unforeseen consequences.

What are some of the technological fixes that are visible on the horizon? I shall list a number of obvious ones. Since those social problems that already have an identifiable technological component are the ones that are most readily dealt with by technological means, I shall first list social problems that have large technological components.

*Air pollution and nuclear energy.* Our air is polluted mainly by the combustion of fossil fuels. Ten million tons of sulfur dioxide pour into the air each year because many of our large central power plants burn soft coal that contains one to four percent sulfur. Hydrocarbons from incompletely burned automobile fuel contribute very heavily to Los Angeles smog. Now it is possible, technically, to eliminate the sulfur dioxide or

the incompletely burned gasoline, but the means are expensive.<sup>1</sup> To catalytically burn gasoline completely to  $\text{CO}_2$  and  $\text{H}_2\text{O}$ , or to redesign the engine to burn gasoline more completely, would add, in effect about seven cents to the cost of each gallon of gasoline; to eliminate  $\text{SO}_2$  might increase the cost of coal by  $10\text{--}25\text{¢}/10^6$  Btu.<sup>2</sup> Thus clean air, which is advantageous to the population as a whole, requires an act of individual sacrifice—paying more for electricity or gasoline. Therefore purification of our air by means of legislation that requires people to use afterburners or by enforcing smoke abatement is slow and uncertain.

Can technology supply a solution that does not require each individual to sacrifice something for the common good—i.e., can technology supply an energy source that is not only cleaner but also cheaper than conventional sources? Until very recently no such possibility was apparent. But now, as the result of a major breakthrough in reactor technology, it looks as though pollution from fossil-fuel burning, central power plants can eventually be eliminated, and at the same time the price of electricity can be reduced. For example, the 2,200 Mw Browns Ferry Boiling Water Nuclear Plant being built by General Electric for TVA is expected to produce electricity at 2.4 mills/kWh, almost 0.5 mill/kWh less than a coal-fired plant at the same site. The power plant will emit no noxious fumes; and even the chemical plant that reprocesses fuel elements can be designed so as to emit no toxic gases. Already 47 major nuclear power plants are in operation, under construction, or committed for construction, and during calendar year 1966, about 65 percent of all central power plant capacity purchased by utilities in this country was nuclear. To be sure, the existing fossil-fueled plants will not all shut down at once. Yet one can anticipate their gradual displacement by the much larger nuclear plants, especially in places (such as Dade County, Florida, or New York City) where air pollution is particularly serious. Moreover, the operating costs of the very large nuclear plants, especially the breeders, should be so low that eventually the older coal-fired plants will be used only as peak-load plants. Thus a technological fix—cheap nuclear power plants—may well lead to cleaner air without requiring much social change.

Another basic cause of air pollution, the gasoline burning automobile, may also be displaced by the cleaner, and possibly cheaper, electric automobile. Here the technological solution is not so clear-cut, though both Ford and General Motors have announced progress in achieving an electric auto. Nuclear power at 2.4 mills/kWh at the bus bar—hope-

<sup>1</sup> To build very tall stacks, as suggested by P. Sporn, to disperse the pollutants from central power plants, would add about one percent to the cost of the power plant. The TVA Paradise #3 stack (~900 feet high) cost \$1.50/KW.

<sup>2</sup> *Energy R & D and National Progress*, prepared for the Interdepartmental Energy Study by the Energy Study Group under the direction of Ali Bulent Cambel, Chapter VIII, U.S. Government Printing Office, Washington, D.C. (1965)



fully at around 6 mills/kWh at the battery-charging station<sup>3</sup>—could play an important part here, since at 6 mills/kWh the energy cost for running the battery-powered electric car should be competitive with energy from gasoline priced at only 10¢/gallon.

*Nuclear Desalination and the agro-industrial complex.* Another major social problem with strong technological implications is water. Here the underlying difficulty centers around the allocation of a natural resource which is abundant in some places, scarce in others. How does one decide whether water is to be used for watering a lawn or for manufacturing steel; or whether the Colorado River is to supply Arizona or Los Angeles? And, indeed, in most discussions of water policy, the implicit assumption is made that our supply of water is limited, and that its use by one group would deprive another group of it. If the waters of the Feather River are used by southern California, they are then unavailable to northern California.

At least part of this attitude toward water policy ought to change as a result of the great advances being made in nuclear desalination. For large cities that are close to the sea, and are not too high,<sup>4</sup> desalted water should be available at about the same price as is now paid for municipal water from conventional sources—around 20¢/1,000 gallons. This is the estimated cost of water (at the plant) from the large Metropolitan Water District nuclear desalination plant scheduled to be constructed off the coast of Los Angeles by 1971. The plant will produce 150 million gallons of distilled water per day (about 15 percent of Los Angeles' total supply) in addition to 1,600 megawatts of electricity that will cost 2.7 mills/kWh. This technological solution to the water problem is now feasible only in very large-scale plants. But the technology is moving fast. For example, the water-moderated reactors that will power the Metropolitan Water District nuclear desalination plant complex might be replaced within a couple of decades by even more economical breeder reactors. And the distillation plant to be used in the Metropolitan Water District project does not include many recent promising possibilities that are still under development such as fluted tubes, or shallow trays, or carbon dioxide for control of scale. When all these possibilities are taken together it seems plausible that water at 15¢/1,000 gallons in large dual-purpose plants (100 million gpd) or perhaps 35¢/1,000 gallons in smaller plants (10 million gpd) will become available. Looking further into the

<sup>3</sup> Six-mill power at the charging station is by no means an unreasonable projection since right now, in the TVA area, residential rates are 7.5 mills/kWh. The figure of 10¢/gallon of gasoline being competitive comes from D. Friedman, "The Correlative Advantages of Lunar and Terrestrial Vehicle and Power Train Research," Society of Automotive Engineers, Automotive Engineering Congress, Detroit, Michigan (January 1966).

<sup>4</sup> If electricity is available at 2 mills/kWh, the cost of the energy needed to raise the water 100 meters is about 0.2¢/1,000 gallons.

future, water from the sea at 10¢/1,000 gallons—a price that is feasible for certain kinds of agriculture—appears to be a reasonable, though difficult, long-term goal.

There are vast social implications of really cheap fresh water from the sea. One possibility is the establishment of a new kind of agro-industrial complex in an underdeveloped, arid land close to the sea—for example, the northwest coast of India. The complex<sup>6</sup> would be based on cheap nuclear energy, and would produce, in addition to electricity for ordinary industrial purposes, water for agriculture at 10¢/1,000 gallons, and ammonia fertilizer (produced by fixing nitrogen from air with electrolytic hydrogen). Some estimates suggest that with nuclear electricity at 1.5 mills/kWh,<sup>6</sup> ammonia could be made for about \$22–\$28/ton which is about half the present cost of ammonia produced in India. Since water in such a complex is costly, one would envisage nearby agriculture being rationalized to the highest degree. It would be necessary to make a very large capital investment and produce food in much the same way, say, as the oil companies produce petro-chemicals and oil in the huge complex near Trombay. Such “food” factories, organized along industrial lines, might circumvent many of the social prejudices that now stand in the way of improving the individual agricultural practices of the Indian peasant.<sup>7</sup>

I have estimated that for  $\$4 \times 10^9$  we could build enough desalting capacity to provide 8 billion gallons of water per day. If this money were diverted, either from our space enterprise or from the world's armament expenditure so that the plants were assessed no fixed charges, then the water would cost less than 10¢/1,000 gallons. At such a price and applying highly rational agriculture, one might use this water to feed an additional  $10^7$  new mouths. Thus to provide water for the 60 million additional persons added to the world population each year would require an investment of around  $\$25 \times 10^9$  annually.

Admittedly, the investment per person to provide the water he needs is high—\$400. Yet in a situation such as this where, in the long run, the stake is long-term stability of the world, it is by no means clear that ordinary economics should determine our strategy. If there are other technologies that will provide water more cheaply, then of course they should be exploited. But traditional cost-effectiveness analysis can be misleading when intangible social benefits are realizable. The nuclear agro-industrial

<sup>6</sup> Such a complex was visualized by R. L. Meier in 1955; *op. cit.*, p. 193.

<sup>6</sup> A price projected, e.g., by J. A. Lane in “Economics of Nuclear Power”, *Annual Reviews of Nuclear Science* 16, 345 (1966).

<sup>7</sup> The Gezira scheme of rationalized cotton agriculture in the Sudan has many of the elements of my proposed agro-industrial complex. See Arthur Gaitskill, *The Gezira, A Story of Development in the Sudan*, Faber & Faber, London (1959). Another example which, however, is primarily industrial, is the huge SASOL synthetic oil complex in South Africa.

complex provides a possibility for integrated development whose economic impact could be cumulative if not auto-catalytic. Its social benefits might therefore reverberate far more widely than one can easily visualize in advance.

Since nuclear agro-industrial complexes are not an unattainable dream, any more than the Metropolitan Water District project was a dream half a dozen years ago. Yet my purpose in describing such a possibility is not so much to urge action as it is to illustrate how a technological fix—cheap nuclear energy coupled with desalination and fertilizer production—could profoundly affect what is viewed primarily as a social problem: the development of a country such as India.

*The safe car and traffic safety.* Perhaps the prototype of the technological fix is the safe automobile. Traffic safety had, until recently, been viewed primarily as a social problem. Laws were passed and enforced, drivers were educated, safety campaigns were launched, and yet the traffic death toll remained high. Ralph Nader's argument—that it is easier to improve the car (a technological problem) than it is to improve the driver (a social problem)—has a kind of transparent logic that I find appealing. As Nader has said, he is concerned with remedies rather than with causes: if one can significantly reduce the number of traffic fatalities by improving the car, even though drivers continue to drive imprudently, this would mitigate the social problem of traffic fatalities.

To the social purist there is something repugnant about achieving remedies by such "tricks," without at the same time rooting out the cause of the social ill. In a sense, however, no social problem is ever completely "solved," except over the very longest period. Were we to wait for the process of education to improve individual conduct to the point where a social problem is eliminated by individual action, we would have to wait much too long. Moreover, the technological approach, through improving the car, by no means precludes campaigns for driver safety or better enforcement of traffic safety laws; nor does it exclude other technological approaches, such as improving highways or traffic control systems.

A general characteristic of the technological fix is that it provides remedies rather than rooting out causes. In the case of auto safety, this aspect of the technological fix is particularly apparent but it is also present in my previous examples. The hydrogen bomb *per se* does not eliminate the causes of large-scale war; it simply makes large-scale war an irrational instrument of policy. Desalting plants do not persuade people to use water more responsibly; they simply provide individuals with more water (at a price to be sure) to do with what they will. Yet the technological fix does more than simply provide a remedy: it often buys time by blunting the social problem. Hopefully, the time so bought so changes

the over-all aspect of the situation, or allows the slow process of education to so improve our "social" behavior, that the original social problems become less intractable. For example, I believe that because atomic weapons maintained a kind of uneasy peace between the United States and the Soviet Union in the 1950's we are able to consider a *détente* in the later 1960's and the 1970's.

*Technological mechanisms for stabilizing the world order.* I turn now to technological approaches to the problem of war and stabilization of the world order. I have already spoken of the H-bomb as a peacekeeper in the sense of making large-scale war irrational. Yet this is not sufficient; if technology is to offer a means for stabilizing the world order, it will have to make small wars irrational, and it will either have to invent some new mechanisms, other than war, for achieving social change, or create a generally affluent world in which pressure for violent social change is much reduced.

Is it at all likely that technology can make small wars irrational undertakings? I cannot answer this question since I have not been involved in our developments of the technology for coping with guerrilla war. The most ominous wars, though, are the wars of national liberation visualized by Lin Piao, in which the underdeveloped masses—the "countryside"—would, by guerrilla action, engulf the developed countries—the "cities." Certain devices that could have a strong impact on such warfare seem to be plausible: like an extremely fast-responding, radar-controlled gun that would greatly increase the risk in attacks by mortar; or a very sophisticated snooperscope that would detect the presence of a person in a jungle 300 yards away; or better transport and communication systems that would enable a well-equipped force to converge upon a trouble spot even more quickly than is now possible in Viet Nam. Obviously, the technological solution to guerrilla or small-scale war is far more complex than the simplistic solution of the H-bomb, and may even be impossible to achieve. And yet, as our experience in Viet Nam seems to show, technology can make guerrilla warfare more and more difficult to wage. Whether it can make it difficult enough to persuade all concerned that some technique other than war must be used to settle human controversy is a moot point that I think must at least be explored vigorously.

The problem of war and of foreign relations is ever so much more complicated than this simple view suggests. Thus, in what sense has the H-bomb made large-scale war an irrational undertaking? Is it sufficiently irrational to dissuade a Hitler with H-bombs or a militant Chinese leader who is convinced that his way of life must be accepted by the entire world to refrain from using bombs? These questions relate to the instability of the bomb-imposed Pax Hydrogenica. Does technology have "fixes" that will increase the stability of the balance of terror?

I can see three broad possibilities. The first is the spy-satellite. A recurring source of friction between the United States and the Soviet Union is the mutual spying that goes on continuously. The U-2 incident brought our relations with the Soviet Union to a low ebb indeed. At the 1955 Geneva Conference we tried hard to come to an open-skies agreement with the Soviet Union, but our attempts were unsuccessful. Yet technology, in the spy-satellite, seems to have developed an instrument for spying that is acceptable to both sides, and that largely circumvents the open-skies issue. One could even visualize spy-satellites being operated by the United Nations or some other international organization, with the data—meteorological as well as military—being available to all. Here is an excellent example of how a technological fix has in surprising degree blunted a social problem that in 1955 and later, especially at the time of the U-2 crisis, appeared to be leading to grave deterioration of our relations with the Soviet Union. I would therefore argue that a more efficient spy-satellite—one that defies attempts to maintain a secretive, closed society—is a most important instrument of peace.

A second means of stabilizing the bomb-imposed terror is effective civil defense, a scheme that probably has stronger social than technological components. If one accepts the view, which I happen to hold, that the major threat of all-out war will come from irresponsible and all but irrational leaders, then any measures that blunt the effect of an attack must be considered a force for peace. A thoroughgoing civil defense by *both* sides, particularly with very short response time so that occupation of shelters would *not* force military action as did the 1914 mobilization, would therefore help stabilize the peace. And, of course, if real disarmament is to come, it seems clear that a civil defense system that would protect against a few clandestine weapons might be an *a priori* necessity.

Finally, I return to the use of technology as a means of raising the living standard among the mass populations of the world. In so raising the living standard, hopefully at a continuing rate of several percentage points per year, one might establish the pre-condition for orderly, rather than violent, social change. I have already alluded to one possibility—the agro-industrial nuclear complex that by-passes the necessity for individual social change, but which of course requires capital. Other examples are the tube-well project to eliminate salting of the irrigated land in the Punjab or the ambitious plans to develop the Mekong River Delta. One can only hope, as the world achieves a kind of precarious stability imposed by modern technologies of war, that money will gradually be diverted to these vast projects—on *both* sides of the Iron Curtain—that can mean so much for the economic development of the poor countries. But I believe that such diversion is no more than a pipe dream

until all war as an instrument of policy has been rejected, and I think this can happen only when all war has become an irrational instrument of policy.

### ***The "Social" Problems***

What about the problems that are much more obviously social and that seem to have very few technological components, such as crime, or race relations, or urban development? Can we in fact discern new technological components in these social problems that enable us to make some progress on them?

Crime is the problem perhaps most amenable to a technological approach.<sup>1</sup> Dr. A. V. Crewe, Director of the Argonne National Laboratory, has pointed out that the resources of modern technology have hardly been tapped in society's attempt to make crime totally unprofitable. For example, Crewe suggests that some of the methods developed for automatic scanning of bubble-chamber plates could be used to identify fingerprints much more accurately than can now be done. With present-day computers, fingerprint records could be stored and retrieved with a precision and detail far beyond current practice. Or, with modern miniaturized electronics, one could mass-produce personal "burglar alarms" that would considerably increase the risk a prowler would have to accept in accosting his intended victim. The technology Crewe envisages for combating crime has much the same flavor as the technology I described for fighting small-scale wars: very fast-response time, new methods of identifying criminals, and possible non-lethal but fast-acting incapacitating agents. Basically the aim is the same—to render crime an irrational undertaking.

Apprehending, say 90 percent of all criminals in cities ought to sharply reduce crime in the city, but it does nothing to eliminate the causes of crime: poor environment, poverty, broken homes, and the like.

Yet reducing the overt expression of these scars on our society can only help, not hinder, the slow and painful process of reducing such social disfigurements. To persuade bitter, frustrated individuals that crime does not pay, to deter them from a life of crime, can only prove helpful to them, as well as to the community that suffers because of their hostility. As with all technological fixes, we shall be buying time that, if we are wise, will be used to root out the causes of our social problems.

I turn now to race relations—a profound, difficult, and most important social problem. Can technology help improve race relations? In one sense, technology has already been helping. Thus our mass communications media—television particularly—now often display Negroes

<sup>1</sup> This possibility was alluded to in President Johnson's State of the Union Address, January 10, 1967. See also, A. V. Crewe, "The Scientific Control of Crime", *Chicago Today IV*, 51-54 (Winter 1967).

as part of our society. Whether it is Willie Mays of the San Francisco Giants or Bill Cosby, the Negro secret operative in "I Spy," or the fellow drinking beer, the appearance of Negroes in everyday settings as individuals, not as Negroes, on our most potent instrument of mass education, ought to have a beneficial effect both on the white image of the Negro and on the Negro image of himself.

But there is an ugly danger that the slow process of social evolution, hastened, to be sure, by technology, may take a disastrous turn because of the bitter, massive actions by Negroes, and the equally bitter counteraction by whites. Can technology help preserve an atmosphere of non-violence so that slow social processes can be given the time needed to take effect? For example, are there socially acceptable technological means of quelling or discouraging rioting? Thus, to put it more constructively, are there technological ways of reducing "police brutality?" I see two rather different technological approaches; the first is related to Dr. Crewe's appeal for more technical sophistication in dealing with crime. I doubt that much very hard, technical thought has gone into the question of whether there are socially and morally acceptable ways of quelling a riot that can give to the policeman an air of gentle firmness. Would, for example, headgear and body armor such as that worn by football players, together with efficient methods of communication, give to the policeman the kind of confidence in his personal safety that would encourage him to deal more gently with would-be rioters? I do not pretend that this is necessarily a workable suggestion (the armor might have the opposite effect of encouraging aggressive behavior by the policeman), but it does illustrate a technical approach to control of civil disorders that might deserve study.

A different possibility would be to offer inducement to refrain from rioting. It was Huntington (1) who pointed out that race riots seem to be correlated with hot, muggy weather. Certainly our racial disorders seem to peak in the summer; and, I would suspect, for two reasons: during the winter it is often too cold to go out; and second, if one lives during the summer in an unappealing, hot apartment, it is more comfortable to be outside. Thus, if the Negro's home surroundings could be improved, especially in hot weather, he would be less prone to spend time on the streets. The most obvious improvement would be air-conditioning equipment; thus, in the various projects to rehabilitate Negro housing—such as the efforts in Wilmington, Delaware—I would give air-conditioning a high priority.

These technological approaches to race relations are aimed simply at reducing violence, at preserving an atmosphere in which social solutions can be worked out. To many they may have too much flavor of the Roman Circus, as a heatless means of keeping the masses subjugated, while the classes attend banquets. Yet I cannot accept this caricature of

America. I am persuaded that we are moving rapidly in race relations, and that one of the main dangers comes from the violence that has been injected into the situation. Let technology render racial violence irrational. I see much more hope for a satisfactory outcome to our racial problems in a calm atmosphere than in one marred by violence.

## II, The Role of the National Socio-Technical Institution

I have tried to show that many of the social problems around which we are now mobilizing have stronger technological components than may at first be apparent. This is not to say I believe that for every social problem there is a technological fix; it is rather than, where a technological fix is available, we ought to get on with developing it as urgently as the importance of the problem demands.

The technological fix will almost always be only a partial answer to the social problem. For example, the problem of civil defense obviously has many important technological elements: can blast shelters be designed to resist fire and bacterial warfare? Can shelters be interconnected so that members of families may be reunited in relative leisure below ground rather than frantically seeking each other above ground? Can the shelter system be designed to serve some constructive civilian purpose, such as conveyance, or parking, or as a utility pipe tunnel? But in addition to these technical questions there are innumerable nontechnical issues: acceptability of civil defense, or predictability of behavior in a shelter, or organization of the population for civil defense. What is needed in a complex, socio-technological matter like civil defense is a "coherent doctrine"—that is, a set of precepts and viewpoints, some from the technological sciences, some from social sciences, some not from science but rather drawn from common sense and experience, that constitute a rational, integrated approach to the problem. In many instances, development of a coherent doctrine would involve, or might even center upon, one or a few pieces of hardware—as for example, the approach to air pollution via development of nuclear energy. In other cases the coherent doctrine would be much less dependent upon the technological fix. The essential point is, however, that the problem under consideration must be looked at as a whole, and the remedies must be sought widely, rather than being unnecessarily restricted to traditional disciplines.

Such coherent doctrine, especially if it contains strong technological components, is best developed in large multidisciplinary institutions whose purpose is clearly the establishment of such doctrine. Examples of such institutions are the Atomic Energy Commission's mission-oriented laboratories—Los Alamos, Livermore, Argonne, Oak Ridge. The weapons laboratories, together with Sandia, have been remarkably successful in their development of nuclear weapons. Moreover, much of the doctrine



we now follow in our deployment of atomic weapons, and the concepts of atomic warfare, can be traced to viewpoints developed at these laboratories. In much the same way, our successes in civilian nuclear energy can also be traced to the multidisciplinary laboratories, along with the Bettis Laboratory of Westinghouse and the Knolls Atomic Power Laboratory of General Electric. The water-moderated line of reactor development, on which almost the entire nuclear energy enterprise in this country is based, traces its origin to early work done at Oak Ridge and at Argonne: the essential features of the system were conceived there, and were then developed by the big industrial atomic power laboratories. Moreover, the development of nuclear energy has required more than technology. Economic and public safety aspects have had to be taken into account in the formulation of over-all strategy, and these, along with the necessary technological consideration, have been synthesized in the reactor laboratories into over-all doctrines.

I recognize several elements in the Atomic Energy Commission laboratories that have played an important part in their success. Perhaps most important, the laboratories, at least in their earlier days, were viewed as institutions rather than as a collection of small projects operated puppet-like from a remote station in Washington. The entire responsibility for achieving the H-bomb, say, was placed squarely upon the Los Alamos Scientific Laboratory. This is to be contrasted with the method of letting a multitude of small contracts from Washington, a practice that is desirable and necessary for basic research or when one is casting about for completely original ideas, but is insidious when aimed at developing a specific device or formulating a coherent doctrine. For coherent doctrines can be developed only by coherent institutions. I can hardly think of a greater catastrophe befalling us than would have occurred had we tried to develop the H-bomb by letting many separate contracts, each of which dealt with a small piece of the entire job. Yet this practice of letting many small contracts to get an applied job done is prevalent now.

If an institution is given the entire responsibility for dealing with a major problem, and if in responding to this responsibility it deploys itself coherently and solidly against the problem, then the institution will need strong and informed management. In some sense, the director of the big institution is the key to the achievement of the coherent doctrine. If he is forceful and energetic and enthusiastic he will be able to mobilize his people around the task at hand. But I believe he should have another quality: he should somehow be in a position to ask, always, whether his entire enterprise makes sense, whether the tasks that the institution is involved in are in fact in the interest of the country. This feeling can come only from contact, personal as well as formal, with those who make science policy in our country. For this reason I would recommend that the President's Science Advisory Committee always have among its mem-

bers a couple of directors of our major national scientific institutions—not necessarily for the insight that they might bring to the deliberations of that group, but more for the education that they will receive and that will be manifest in the relevance to our Nation of the activities of the laboratories these directors lead.

But, if big laboratories are to attack social problems, even those susceptible to technological fixes, they will have to acquire more appreciation of the social components of such problems. Thus I would visualize mission-oriented institutions that combine the characteristics of RAND or the Brookings Institution and those of Los Alamos. There would be both “hardware” and “software” types, with the former exploring technical means of achieving socially desirable ends, and the latter investigating the consequences of such technical inventions, posing social questions, perhaps articulating the coherent doctrines that are developed by the institution.

What I am describing is really not so very different from some existing institutions; the Stanford Research Institute perhaps comes most readily to mind. Many corporations already have this interplay—between market analysts on the one hand and, say, insecticide chemists on the other. My proposal amounts to establishing, in those areas of *social* concern that clearly have technological components, multidisciplinary institutes that address themselves to these problems. Thus as new agencies with broad social responsibilities, like the Water Resources Council, or the Environmental Science Services Administration, or the Department of Transportation, or the Department of Housing and Urban Development are set up, one of their first tasks ought to be to establish the nuclei of such multidisciplinary laboratories. And, of course, the first task of such nuclei would be to see whether there are points of departure, particularly with technical content, that the laboratories can get their teeth into.

Can existing mission-oriented, multidisciplinary laboratories be redeployed to advantage around these social problems? Fully \$4 x 10<sup>9</sup> of the Federal Government's research and development budget goes for support of all these Government laboratories, and, if some of them could be redeployed in this manner, we would be the better for it. However, I can see three difficulties in such redeployment. First, some public opinion to the contrary notwithstanding, most of these laboratories are still heavily involved in matters of the utmost importance—like, for example, development of the breeder reactor. Second, an existing laboratory may not have the necessary skills to redeploy: could Argonne take on the job of crime, or NASE-Houston the job of developing the artificial heart? Actually I am rather optimistic in this regard. These laboratories tend to be isomorphic; they all have physicists, chemists, engineers, and mathematicians. Some of them have biologists, and a few even have social scientists. Naturally, in any redeployment, jobs ought to be assigned where skills exist or

can be mobilized. Thus Sandia, a fine weapons laboratory, might take on the important job of small-scale warfare, or Lincoln Laboratory the job of improved surveillance. We at Oak Ridge have become a full-fledged water laboratory without having to hire an appreciable number of chemists or engineers: heat transfer in a multistage still is similar to heat transfer in a boiling water reactor, and the rejection of salts by membranes is closely allied to chemical separation by ion exchange.

But there is another, perhaps more serious, difficulty in redeploying our Government laboratories around the new set of social problems. We really do not have any *Government* laboratories. An agency laboratory is supposed to work on missions assigned to its sponsoring agency by Congress. But agency missions have a way of becoming obsolete, especially, as Harvey Brooks puts it, when the agency is organized around a technology. The National Aeronautics and Space Administration possesses many of the finest laboratories in the country; what is to become of this apparatus after the Apollo mission has been completed?

As matters now stand, it is dangerous for an agency laboratory to deploy itself too strongly around problems that are of interest to an agency other than its own. For, in so doing, the laboratory's action might be interpreted, often incorrectly, as suggesting that the agency for which it is working either is no longer very important, or that it has more scientific resources than it really needs. Moreover, if a laboratory allows itself to become truly "national" by redeploying around urgent problems outside its agency's own responsibility, it may soon find itself in a position in which no agency feels responsible for it *as an institution*. And, as I have already stressed, coherent doctrines are framed only in coherent institutions: if the institution becomes a collection of separate, precariously supported pieces, there is little likelihood that it will develop a coherent anything—either doctrine or hardware!

Yet precisely herein lies one of the great strengths of the multi-disciplinary "national" laboratory. By virtue of its position as a developer of a coherent viewpoint, it can reintegrate at the working level the parts of a national problem that so often become fragmented among many different agencies or different parts of the same agency. The most rounded and most coherent view of a complicated techno-social problem often resides in the expertise in the laboratory rather than in the central headquarters. This integrative function of the laboratory is to my mind a most precious attribute of these institutions, one that we should try hard to preserve and promote.

Despite these difficulties, some redeployment has already occurred. And indeed in its 1960 report to the Joint Committee on Atomic Energy, the U.S. Atomic Energy Commission stated: "From time to time, the Commission will utilize these laboratories . . . for urgent tasks . . . of importance to the Nation." Some of the Atomic Energy Commission

laboratories have undergone a partial redeployment: the Oak Ridge National Laboratory, for example, is now an arm of the Office of Saline Water, of the National Institutes of Health, and the Office of Civil Defense. Although we have had some rough going in our efforts to redeploy, I believe the redeployment, on the whole, has been successful. I would therefore urge that such rather informal redeployment be encouraged wherever it seems to be expedient. In most cases the judgment of expediency can best be made by the laboratory management. Eventually one would hope that, after many more laboratories have redeployed informally, some Government-wide policy (perhaps involving a holding company for Government laboratories such as was suggested in the Bell report) can be formulated to give these redeployed laboratories a home in Government.

The other possibility would be to create new *ad hoc* laboratories devoted to each of these newly identified social problems. Whether in any given instance a new institution is better than a redirected older institution one cannot say. I suppose I have an aversion to the creation of new laboratories, especially when I see fine older Government laboratories preoccupied with matters that are no longer, it seems to me, as centrally important as they were when the laboratory was created. On the other hand, new organizations have the advantage of tending to break up old habits of thought, and of bringing fresh blood into top management. Yet I think the issue cannot be prejudged. The important point is that big public problems, and this includes big social problems that have strong technological components, are best handled in big, mission-oriented, Government-supported laboratories. Whether we set up new laboratories or redeploy older ones is a matter of tactics to be decided case by case.

I have said nothing about the role of the university in the resolution of these big problems. Of course the university will participate. Individual professors and their students ought to be enlisted to help, particularly in the most delicate and deepest thinking that must underlie any "coherent doctrine." And within the universities a coterie of specialists must be created who will be prepared to devote their lives to the relevant fields. But I cannot visualize the university, as an institution, taking the responsibility for, say, the problem of crime, or of civil defense, or of urban renewal. This does not in any sense preclude the establishment of mission-oriented, multidisciplinary laboratories as adjuncts of universities like, for example, the Jet Propulsion Laboratory at the California Institute of Technology, or the Lincoln Laboratory at the Massachusetts Institute of Technology. But the connection with the university is peripheral, not central. The laboratory with responsibility for a given job must be ready and able to focus wholeheartedly on the job. To the extent that a connection with a university helps the laboratory get on with its mission, as by enabling it to recruit better people, the connection is valuable. To the

extent that the connection with the university injects an inappropriate disciplinarity into the laboratory, I would hold it to be disadvantageous.

Nor have I mentioned the role of "private" industry in this mobilization around these broad questions. I use quotation marks around the word "private" because most of the issues we are now addressing, being social, hardly lend themselves to profit, at least initially; private, profit-making industry typically serves as a management agent rather than a purveyor of goods in these situations. But the real difficulty is that the means available to enlist private industry on public problems, the short-term contract, simply is too fragile to work well on open-ended problems whose solutions are never very clear-cut. How do we know when our cities are renewed; or our civil defense adequate; or our crime rate acceptable? Thus, there are few criteria available for determining in such situations whether or not the private firm is doing a good job. Under the circumstances I should think there might be a tendency to give the contracting officer the answer he wants, rather than developing independent, and possibly unpopular, "coherent doctrine." Thus, insofar as such doctrines can be developed best in a viable institution whose existence is not always at stake (as is the case with many small, private "think-factories"), I would argue that, on the whole, these institutions should be set up as long-term, Government-owned entities. This doesn't mean that they could not be operated under contract by an industry, or a university, or a combination of universities. But the exact arrangement in this regard is a secondary matter.

### III. Dangers and Second Thoughts

The burden of my argument is that, in attacking social problems, we ought always to strive for establishing what I call "coherent doctrines." The easiest coherent doctrines are those that can be embodied in the technical fix. Insofar as such technological fixes can be *a priori* identified, their development can best be accomplished in big national laboratories. In cases where the technological fix cannot be *a priori* identified, it is still worthwhile to deploy technologically oriented institutions around social problems, since modern social problems, almost without exception, have some technological components, and these components are best identified in the environment of a big laboratory.

Are there any dangers in following such a course? The first, and most obvious, is that by placing responsibility in a big technologically oriented institution we may overestimate the importance of the technological component of social problems. As the social scientists are fond of saying, the technologists are too "simplistic" in their approach to social problems; technology can never replace the arduous job of the social engineer. And of course this is true. It is for this reason that I visualize these national laboratories for social problems being seasoned, especially in the

higher management, with software as well as with hardware types. One would hope that each could keep the other honest.

There is another danger. Will the laboratories—the developers of coherent doctrine—become too powerful? This is not an idle concern; I believe that some people, both within and without the armed services, consider the RAND Corporation to be dangerous simply because it is so successful. Many of our strategic doctrines, and certainly much of the language in which the dialogue concerning strategy is conducted, can be traced to RAND. Since these matters touch upon some of the most sensitive areas of our society's concern, it is somehow disconcerting that they are formulated by experts who, at least from the outside, appear to sit apart and to operate on their own.

I suppose I have only one response to this sort of concern: establish in each case not one but rather two competing institutes that will keep each other honest. This worked very well with Livermore and Los Alamos, and with Oak Ridge and Argonne. I should think it would work well in every case in which the issues of concern are sensitive, and where, because of technical complexities, they cannot easily be subjected to public debate.

And finally, I would leave a different word of caution. In committing ourselves to serious attacks upon our great social problems, we have perhaps unwittingly assumed that the methods of science—of analysis, of objectivity, of sharp definition—are going to work. But in this we make a great and unprovable assumption. As a physical scientist I can only say that physics and chemistry and engineering have worked in the past to solve technical problems. Whether science will also work for social problems, even those seeming to have technological components, cannot always be ascertained *a priori*; all we can say is that thus far we have no alternative to the hypothesis that science is effective. Yet we must not confuse putting a problem into the laboratory for solution with getting a workable solution to the problem. It would be tragic if we became so enchanted with our techniques and technologies, our technological fixes, and our coherent doctrines that we neglected to make as much progress as we could through the traditional instruments of Government and by our own good common sense.

For, in the final analysis, we shall have to depend on our good common sense. I recognize that technological "fixes" include such monstrous perversions as the ovens at Dachau—that a coherent doctrine can become a perverted doctrine if the instruments of power are captured by evil men. There is always danger that somebody will seize upon a partial truth developed by science to serve as the basis of a coherent doctrine that is tragically in error. National institutions that combine broadly the viewpoints of both the natural and social sciences, and in which the differing viewpoints are allowed to compete, would seem to me to be less suscepti-

ble to being captured by a pet and erroneous doctrine than would inbred Government bureaucracies. But one still must have faith that ultimately our tradition of decency and enlightenment will thwart any among us who are tempted to misuse the fruits of our new socio-technology, and that, on balance, we shall continue to move toward a more humane and rational society.

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